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THE LOWELL SPECTROGRAPH.

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IN order to take advantage of the superior atmospheric conditions for spectroscopic researches at this observatory, Mr. Lowell, the director, ordered of John A. Brashear, in December 1900, a complete spectroscopic equipment as efficient as could be constructed. It was to be used in connection with the large Clark refractor and, pursuant to the policy of this observatory, to be devoted primarily to planetary investigations. The work to be undertaken lay along two lines: (1) to determine the rotation periods of the planets, and in particular that of *Venus*, by measurements of the Doppler displacements of the spectral lines; and (2) to study the atmospheres of the planets by comparison of their spectra with the solar spectrum. The first of these problems demanded a powerful spectrograph, as such work could be done only photographically; and the second required a universal spectroscope with which observations could be made both visually and photographically. And to deal advantageously with the light from objects as bright as *Venus* and as faint as *Neptune* and the brighter satellites of the solar system, it was necessary that the equipment should be capable of yielding spectra varying considerably in dispersion. These conditions are well satisfied by a powerful three-prism spectrograph, supplemented by apparatus carrying either of two single 60° prisms differing widely in dispersive

power, or a Rowland plane grating, and supplied with cameras and telescopes of different focal lengths. Plate II shows the spectrograph mounted on its laboratory stand and surrounded by the other parts of the equipment.

The order to Mr. Brashear was for the best spectrograph he could make. In consequence, he consulted those using his spectrographs in order to obtain any suggestions which might assist in designing and building an instrument, the efficiency of which would be the highest attainable. In design and mechanical construction it is similar to the Mills spectrograph of the Lick Observatory. It differs however, in having a collimator truss of three rods instead of four, a Huggins guiding device, and a prism-train equipped with a minimum deviation device. In planning and building the instrument, care was taken to insure great rigidity throughout, and particularly in the prism-box, and the construction is sufficiently massive to insure against flexure.

The large refractor for which the spectrograph was designed and with which it is used has an objective, by Clarks, with a clear aperture of 61 cm and a focal length of 983 cm for the photographic rays. This ratio of aperture to focus—1 to 16—had to be satisfied by the collimator lens in order that it might transmit all the cone of rays from the large objective. The length of the collimator depended upon what seemed to be the most feasible aperture and dimensions to give the spectroscope. The amount of weight that might be added to the telescope tube without danger of flexure, and the additional length of tube that the dome could well accommodate, together with a consideration of the efficiency of the instrument for planetary work, led to the adoption of a collimator of about 490 mm focal length. This gave the lens an aperture of 30.5 mm, and defined the size of the prisms and the dimensions of the spectrograph.

The spectrograph was mounted in the autumn of 1901. Complete, with the necessary counterbalances at the upper end of the telescope, it added 450 pounds to the weight of the telescope tube, which required a corresponding increase in the counterbalances on the declination axis. The mounting of the refractor is massive, and this considerable increase in the weight upon the bearings has not influenced the running of the driving-clock. To counterbalance the

spectroscope at the objective end, the rack and weights accompanying the spectroscope were mounted, but were soon removed and the tube wrapped with heavy sheet lead held in place by two ring clamps. This made the weighting symmetrical, and the telescope is in equilibrium in all positions. This counterbalance is permanent, and when the spectroscope is removed it is necessary to add an equal weight of counterbalances to the racks at the eye-end.

The attachment of the spectroscope to the telescope is as follows: A large conical-shaped cast-iron jacket, with a flange, fits over and, near its circumference, against the heavy end-casting of the telescope tube. Three large capstan-headed screws fasten the jacket to the end-casting. They are each accompanied by a pair of butting screws which, together with three on the flange for lateral adjustment, serve to set the axis of the spectroscope into coincidence with the optical axis of the refractor. The jacket tapers rapidly from 27 inches at the flange down to 13 inches at its lower end, where it supports a second casting, a cylinder which has at its upper end a flange through which six capstan-headed screws pass and rigidly join it to the jacket by a bayonet joint. The lower end of this cylinder is surrounded by a collar upon which the spectroscope proper is supported. Three hinges, mounted on the upper end of the collimator section which fits against the cylinder, swing in over the collar and clamp by milled-head screws. This gives a convenient and yet rigid attachment of the spectroscope to the refractor.

The jacket is intended to be left on the telescope. The cylinder is readily removable, but, as it does not in any way interfere with the tail-piece of the telescope when equipped for micrometric work, it is left in place on the jacket. In changing from visual to spectroscopic work, the section of the tail-piece carrying the micrometer and the focusing rack and pinion is removed, and the tube of the correcting lens is inserted in its stead. The spectroscope is then attached, and the electrical connections of the temperature control and comparison apparatus are made. The convenient arrangement of the different attachments makes the change from one kind of work to the other require only a few minutes' time.

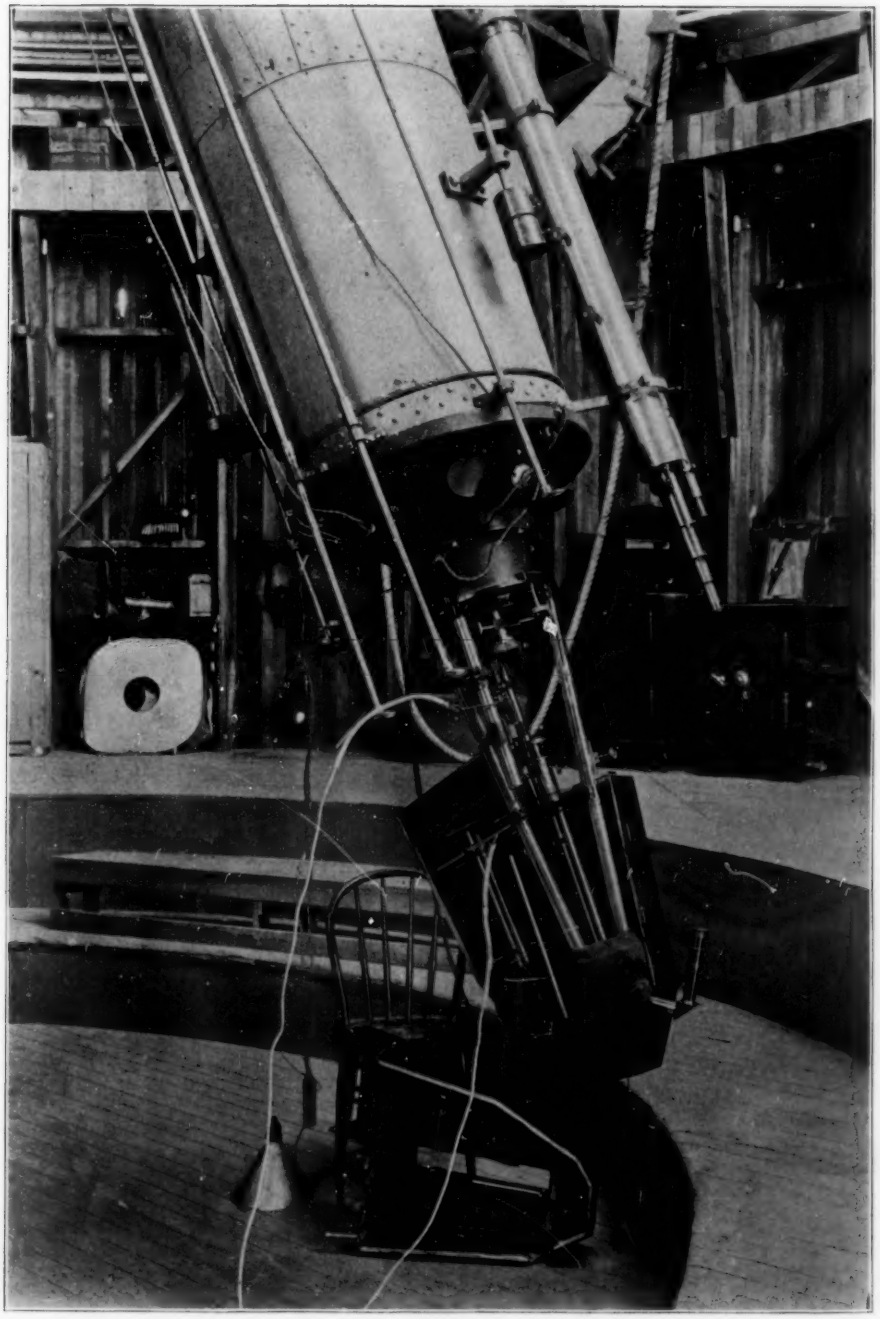
The lower end of the cylinder of the adapter carries a position circle graduated to degrees, and the end-casting of the spectroscope

has an index fastened to it so that by loosening the milled-head screws of the clamps the entire spectroscope may be rotated through any desired angle about the optical axis of the refractor. This feature of the mounting has been of great convenience in work on the rotation of planets. It permits the spectroscope to be turned so that the camera is either above or below the collimator—a change which is frequently desirable for ease in guiding.

In a mechanical sense, the principal part of the spectrograph is the collimator section. It consists of three hollow steel rods, $1\frac{1}{4}$ inches in diameter, two end castings, and an intermediate web, all united into a rigid tripod-shaped truss, which is 31 inches long and weighs 51 pounds. The ring casting at its upper end, 12 inches in diameter, carries the clamps which attach the spectrograph to the adapter. The lower end of the truss, $6\frac{1}{4}$ inches in diameter, supports the prism-box. A sleeve firmly held by the web and the lower casting incloses the collimator tube. The collimator and slit-plate may be moved as a whole in this sleeve through 70 mm by a rack and pinion, to bring the slit into the focus of any desired ray from the objective. A millimeter scale and clamp are used to set the slit in any desired position. The upper end of the collimator tube projects beyond the sleeve and is supplied with a focusing rack, a clamp, and a millimeter scale for fixing the slit in the focus of the collimator lens. An arm, 15 inches long and $1\frac{1}{4}$ inches in diameter, which is attached to the upper casting, carries the guiding tube and the sparking apparatus for furnishing the comparison spectrum.

The slit of this instrument is quite similar to that of the Mills spectrograph, save that it is arranged for guiding by the Huggins method. The jaws are of speculum metal and are highly polished. Both are movable. A screw drives a wedge which opens the jaws symmetrically, and springs close them as a backward turn of the screw withdraws the wedge. The screw has a pitch of 0.25 mm and a milled head divided into ten equal parts; thus one division corresponds to a slit-width of 0.025 mm. A wedge-shaped metal tongue is mounted immediately in front of the central part of the slit. It is supported by an arm sliding in ways at the side of the jaws, and is supplied with a block and screw to limit its motion. In this way the portion of the slit occupied by the stellar source may

PLATE I.



THE LOWELL SPECTROGRAPH ATTACHED TO THE TWENTY-FOUR INCH REFRACTOR.

be conveniently covered while the terrestrial or comparison spectrum is photographing. Just beneath the slit is a pair of jaws for controlling the width of the spectra. By turning a milled screw, accompanied by a scale, these jaws are opened and closed symmetrically.

The prism-box is semicircular in shape, 13 inches in diameter, and $2\frac{5}{8}$ inches deep. The heavy brass base-plate is cast with a perpendicular section along its straight edge, braced beneath by two ribs. To this section is firmly united a thick circular disk worked to fit the end-plate of the collimator truss. Through the disk pass four capstan-headed screws which rigidly attach the prism-box to the collimator section. The circular wall and top of the box are of brass, and all are fastened in place by screws.

For the purpose of adapting the spectrograph to work in any part of the spectrum, the prisms are mounted with a device for keeping them automatically in the position of minimum deviation. The framework of the device ends in a rectangular box, the outer end of which receives the camera tube. The sides of the box, toward the inner end, are cut away for some distance. This allows the top and bottom to pass above and beneath the third prism, where they are securely clamped, respectively to the top and base of the prism-box. Milled-head screws passing through the cover of the prism-box press lightly on the mountings of the prisms, holding them in position against the base. The box, complete with the prisms, weighs $21\frac{1}{2}$ pounds.

The camera tube screws into the prism-box mechanism, with the lens near the third prism. Its upper end is held in place by two rods which extend from the web of the collimator section and clamp to the tube. The instrument is supplied with two cameras, 15 and 19 inches long, respectively. Their tubes are of brass, $2\frac{1}{2}$ inches in diameter and are supplied with focusing racks and millimeter scales. The long camera is equipped with a plate-holder receiver arranged for tipping the photographic plate, and with slides for occulting parts of the spectrum that tend to become overexposed. The plate-holders, four in number, are of brass and hold plates of size 2 by 3 inches. An eyepiece made to fit in the camera instead of the plate-holder is found extremely useful for viewing the spectrum to test the

performance of the spark, and to judge the exposure time for the star as well as to test the guiding. It is kept in reach, and when the plate-holder is withdrawn it is slid into place and left until the next exposure is to begin.

Two guiding devices are provided. One utilizes the light stopped by the jaws of the slit; the other, the light which is reflected from the face of the first prism. The highly polished speculum slit-jaws are sufficiently inclined in the same plane to return the reflected light just outside the cone of rays from the large object-glass. A totally reflecting prism intercepts the reflected beam about 6 inches above the slit and directs it into a guiding telescope which stands at right angles to the optical axis of the refractor and is mounted on the arm extending from the upper end-casting of the collimator truss. The field of this guiding telescope is such that it includes the star when it has been brought into the center of the field of the 4-inch finder, and the star is quickly set on the slit by turning the slow motions. In the guiding tube one sees the image of the star or planet with the slit crossing it as a dark line. The center of the slit is marked by drawing the occulting tongue until its point is nearly in contact with the dark image of the slit. This pointer and a reticle in the tube enable one to guide very satisfactorily. Accurate guiding is more necessary in planetary than in stellar work, for it is generally required that the light entering the slit should come only from some particular part of the planet's disk. It is in this work that this method, first used by Huggins, is practically indispensable.

By the other method, due to Vogel, the light reflected from the first face of the first prism passes directly into a guiding tube mounted on the wall of the prism-box. The tube is constructed with an elbow so that the observer may rotate the eye-end into a convenient position for guiding. Although the guiding is done principally by the Huggins device, this one is always used to time the exposure when the spark spectrum is photographing, and, by removing the eyepiece, to see whether the whole of the collimator lens is illuminated by the spark.

The sparking apparatus is mounted on the arm which carries the Huggins guiding tube. It is supplied with a device for holding the metallic electrodes, which is readily replaceable by one carrying vacuum tubes. A condensing lens of wide field is mounted between

the spark and the slit-plate. It throws an image of the spark on a totally reflecting prism which directs the light into the collimator. The spark-gap for the tubes and the electrodes is parallel to the collimation axis.

A large induction coil capable of a 12-inch spark in air gives the high potential current for the spark. It receives current from a 104-volt alternating current with 133 complete oscillations per second and of about one unit amperage. Three one-gallon Leyden jars in the secondary circuit serve to suppress the air spectrum. A rheostat in the primary circuit is used to adjust the current to suit the spark-gap so as to give a clear metallic spectrum.

For protection against temperature changes the entire spectrograph, except the slit and guiding tubes, is inclosed in a wooden case. As Plate I shows, the case is wide at the top to permit work in the less refrangible parts of the spectrum. It is made in two sections which fasten together with hooks and eyes. A heavy clamp, half of which is firmly mounted on the upper end of each section, takes hold of one of the truss rods just above the web near the upper end of the collimator tube. This supports the entire weight, 22 pounds, of the case and holds it free from the spectrograph. The box is made of red cedar and lined throughout with layers of corrugated and felt paper covered with heavy silk plush. A hinged door over the end of the camera permits the spectrum to be observed and gives access to the plate-holder.

An electric heater containing 30 feet of No. 20 German silver wire is mounted in each half of the case, opposite and extending above the semicircular base and top of the prism-box. The heating current is led in from the mains of the 104-volt alternating current with which the dome is lighted. An incandescent light, with bulbs of different candle-power, is placed in series with the heaters, and a finer regulation of the quantity of current is effected by a twenty-one step rheostat, while a switch in reach of the observer starts and stops the heating current. Thus any desired quantity of current is at hand, and the maintenance of the prisms at a constant temperature is rendered easy.

A large-scale thermometer mounted with its cylindrical bulb along the base of the second prism records the temperature within the

prism-box. Its scale is a little greater than 1 mm to 0°.1 C., so that it can be read with ease to 0°.02. The temperature of the air-space within the wooden box is read from a second thermometer mounted opposite the first. Both are read through a window. The prism-box is additionally protected by three thicknesses of heavy woolen cloth. Thus a change of temperature shows itself in the outer thermometer in time to alter the current before the effect reaches the prisms. In general, the temperature does not change more than 0°.1 or 0°.2 C. during a night's observations, and it rarely happens that the variation is more than 0°.1 C. during an exposure of two or three hours.

After the spectroscope had been mounted and the adjustments had been made, in the spring of 1902, the first work undertaken was a determination of the color-curve of the 24-inch objective. The determination was made visually with the prism-train. Later, in the summer, the blue and violet part of the curve was redetermined photographically by use of a single dense prism. This gave the photographic part of the curve quite accurately and in close agreement with what the visual observations had made it. The color-curve is reproduced in Fig. 1, I. That part of it to the red of $H\beta$ depends upon the visual observations; the other end of the curve is principally from the photographic data. As may be seen from the curve, when the slit of the spectroscope was placed in the focus of the $H\gamma$ rays, it was about 20 mm inside the focus of the $H\delta$ rays and 19 mm outside the focus of the $H\beta$ rays. Thus the cones of the $H\delta$ and $H\beta$ rays from a star would have a diameter of about 1.2 mm where intersected by the slit-plate, so that the slit (0.02 mm wide) could admit only a very inconsiderable part of those rays, and the star spectrum would decrease in intensity quite rapidly at short distances on either side of $H\gamma$, particularly on the violet side, where the curve was steeper and where the absorption of those rays by the large objective and the lenses and prisms in the spectroscope entered to decrease the light intensity. Stellar plates, therefore, varied from overexposure in the center to weakness toward the ends, making them unsuitable for the most accurate measurement. In the case of the planets where there is a considerable disk, this decrease in the intensity of the spectrum was much less serious; *Jupiter's* spectrum, for example, extended the

full length of the plate (λ 4700 to $H\delta+$), though weak toward the violet.

It was evident that a distinct gain would result in all work if the color-curve were made flat through the photographic part of the spectrum. A copy of the curve was sent to Mr. Brashear. From

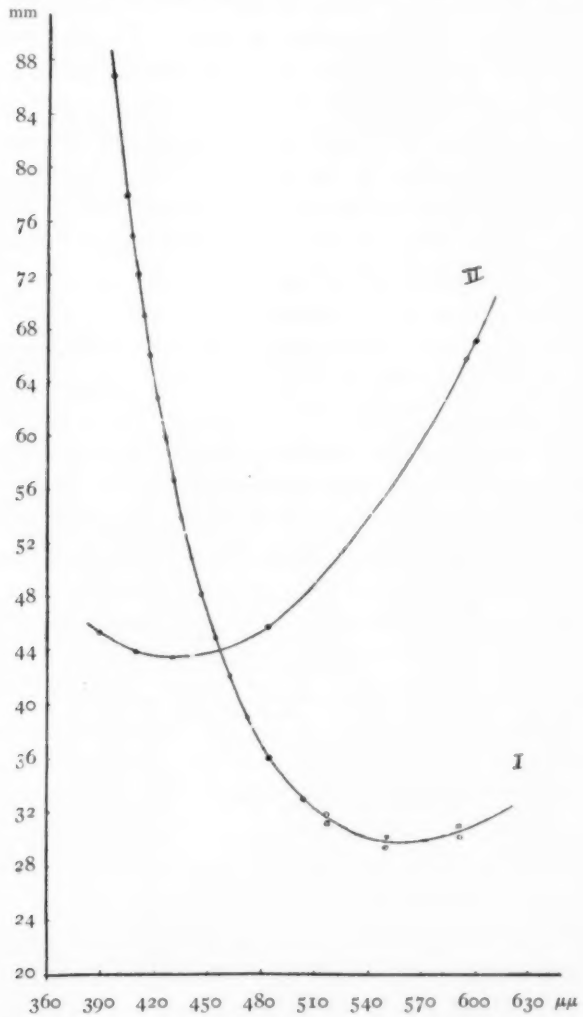


FIG. 1.—Color-Curve of 24-Inch Objective: I, without Correcting Lens; II, with Correcting Lens.

it Dr. C. S. Hastings computed a correcting lens which would unite upon the slit-plate the photographic rays as far on either side of λ 4384 as was feasible, for which ray the lens was not to change the angular aperture of the 24-inch glass. Since the planetary spectra in general fall off rather rapidly in intensity to the violet side of λ 4315, it seemed advantageous to make the central ray near λ 4400, and the correcting lens was constructed accordingly. The same decrease in brightness toward the violet is likewise true of the spectra of all solar-type stars; moreover, the most important lines for velocity work in the spectra of stars of the earlier types are comprised between $H\gamma$ and λ 4481, and consequently the choice of the central ray is equally advantageous in any stellar velocity work that might be undertaken.

Since the lens needed a somewhat larger undistorted field for planetary work than is required in stellar work, it was made larger than is usual. It has an aperture of 95 mm and is placed 1.422 m inside the focus of λ 4400, which light it brings to a focus about 8 mm nearer the object-glass. The lens is a doublet made of a double convex lens of a special light flint from Mantois, with very slight absorption, and a light crown lens, double concave in form. The two components are cemented together to avoid loss of light by reflection. The lens is mounted in a stiff tube 4 feet long, which slides by rack and pinion in a second tube having a threaded collar 5 inches in diameter which screws into the second joint of the tail-piece of the telescope in the place of the micrometer adapter. This gives the lens a stable mounting. The threads of this joint are "interrupted," and the lens and section of the tail-piece may be quickly interchanged.

The lens renders a star spectrum practically linear from $H\delta$ to λ 4700, *i. e.*, for 300 tenth-meters on either side of λ 4400. With it, the focus of $H\gamma$ is less than 1 mm shorter than that of $H\delta$ and about 2.5 mm shorter than that of $H\beta$. It transforms the color-curve of the 24-inch glass into II of Fig. 1.

The lenses of the spectrograph, four in number, are composed of crown and flint glass from Mantois, Paris. The construction, which is the same for all, is a doublet lately designed by Dr. Hastings, with the inner (contact) surfaces cemented.

There are two collimation lenses, each having an aperture of

30.5 mm. The one designed for work on the photographic rays is corrected for $H\gamma$; the other, to be used in the lower part of the spectrum, is correspondingly corrected.

The focal length of the former lens was determined in the laboratory. After the slit had been placed in the focus of the lens, by Schuster's method, and the short camera focused for parallel rays, the camera objective was directed into the collimator objective, and a negative of the spectrum of iron was pressed with its film against the jaws of the wide-open slit, illuminated, and its image in the camera photographed. Measurements under the microscope of the relative linear scales of the original spectrum and its photographed image gave the collimator a focal length 1.2683 times that of the camera, or 489.1 mm. When the photographic lens was replaced by the visual lens and the slit placed in its focus for the yellow rays, it was noted that the collimator scale-reading was increased 2.2 mm, and therefore that the focal length of the visual lens was 491.3 mm. Thus the angular aperture of the lenses is 1 to 16, the same as that of the 24-inch object-glass.

There are two camera lenses, each of 36 mm aperture, but differing considerably in focus. Their focal lengths were determined by star trails. The measurement of a plate of trails of the *Pleiades* gave the following values for the focal length of the long camera:

Star 16	*Tauri*	$f = 471.4$ mm
17	"	471.7
19	"	471.1
20	"	471.5
21	"	471.4
22	"	470.9
23	"	471.4
25	"	471.5
27	"	470.9
28	"	471.6
1170	*B. A. C.*	471.6
	Mean	$f = 471.4$ mm

This makes the angular aperture of the lens 1:13.

Measures on a plate of the same group of stars gave 385.7 mm for the focal length of the short camera. Its angular aperture is 1 : 11.

It has been found that, with the long camera, a greater length of spectrum is sharply defined if the plate is tipped to bring the violet

end nearer the lens. As the tube is provided with a device for tipping, this causes no inconvenience. This lens appears to perform somewhat better at about λ 4500 ($H\gamma$ in center of plate) than does the short camera, while the latter lens seems to define more sharply to the violet side of λ 4250 than does the long one.

For visual observations of the spectrum, two telescopes are provided, each of 33 mm aperture. The focal length of one, as determined by star trails, is 496.4 mm; of the other, 255 mm. An excellent little micrometer which may be used with either telescope meets all needs for visual measurements. The screw has a pitch of 0.5 mm and a head divided into 100 parts.

In ordering the glass for the prisms from Messrs. Schott & Co., Jena, Mr. Brashear gave special instructions for homogeneity and fine annealing. Consequently the glass was cast in a block from which the prisms were sawed so as to secure symmetrical annealing and as high a degree of homogeneity as possible. The glass is a dense silicate flint, the same brand, O 102, as that used in the new Potsdam spectrograph, No. III. This glass was selected on account of its high degree of transparency and freedom from color. The three prisms were given the same dimensions, but, as they are not very large and are almost free from absorption, this does not occasion any considerable loss of light. The bases are each 67 mm, and the average length of the path of the light through the glass is slightly more than 100 mm. The angles and the minimum deviations of the prisms were measured here a few months ago. The results of the measurements and the corresponding indices of refraction are given below.

TABLE I.

Prism I, angle = 62° 52′ 56″
Prism II, angle = 62 52 12.
Prism III, angle = 62 52 31.

Line	3 D	n
$H\alpha$	165° 22′	1.64344
D_2	167 18	1.64893
$H\beta$	172 11	1.66268
λ4529	174 41	1.66963
$H\gamma$	176 29	1.67448
$H\delta$	179 14	1.68191
H	181 5	1.68680
K	181 38	1.68826

The average index for $H\gamma$ is 1.67448, and the total deviation is 176° 29′.

The measured angles and the indices of refraction of the prisms were found to be somewhat too small for the dispersion the instrument had been giving. The prisms were readjusted for minimum deviation, and the dispersion is now what it should be from theoretical considerations. It was understood that all adjustments had been carefully made and the prisms fixed in a permanent position in their box before the instrument left the maker's hands; and as an examination showed that the prisms had not been appreciably displaced in transportation, work was consequently begun with the instrument without testing the adjustment of the prisms for minimum deviation. Inasmuch as it had been my habit to test frequently the focus of the collimator lens, the small amount the prisms were out of the position of minimum deviation at $H\gamma$ could not have sensibly affected the spectrum otherwise than to somewhat increase the dispersion.

The prisms have not been examined individually, but the prism-train has been tested as a whole. The tests were made visually on the solar spectrum. While the writer observed the spectrum, an assistant occulted one-half the collimator lens or diaphragmed it down to half its aperture, so that the light was made to pass in turn through the half of the prisms near the refracting edge, through the half near the bases, and through the central half of the prisms and lenses. It was thus found that the definition of the spectrum was best when the diaphragm had been used, and less good for the half near the bases than for the half near the edges. No difference could be detected in the definition when a half of the prism-face was occulted at right angles to the refracting edge. Different collimating and observing lenses were used, so that the differences are due largely to causes in the prisms, probably to a lack of homogeneity in the prism-glass. But, unless all the optical parts are perfect, some such differences in the definition are to be expected.

It was noted also that a beam of light passing through the part near the edge or base of the prism-train did not focus quite at the same point that it did when passing centrally through the lenses and prisms. This, too, seemed to indicate a lack of homogeneity in the

glass, which, however, may be in part, probably not wholly, attributed to the rapid and considerable rise in temperature of the prisms, during the observations, occasioned by opening the dome and removing the temperature control and the top of the prism-box and turning the instrument on the Sun. It is therefore probable that the glass is not quite homogeneous, but perhaps it is as nearly so as it is possible to cast it.

A comparison of the angular and linear dispersion, at $H\gamma$, of this and the other largest spectrographs is to be found in the accompanying table:

TABLE II.

Spectrograph	Dispersion		
	Focal Length of Camera	Linear, Tenth-Meters per mm	Angular, for one Tenth-Meter
Lowell	S 386	14.5	36″.8
Lowell	L 471	11.4	36.8
Mills (Lick)	406	12.6	40.5
Potsdam III	1 560	10.2	36.5
Potsdam III	2 410	13.8	36.5
Bruce (Yerkes)	A 449	10.7	42.8
Bruce (Yerkes)	B 607	7.9	42.8

Since the Bruce prism-train is set at minimum for λ 4480, where the dispersion is somewhat decreased, the four spectrographs have practically the same dispersive power.

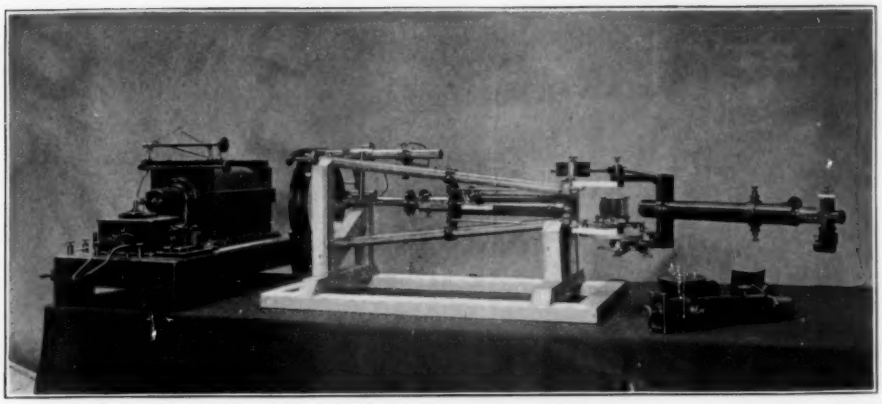
The linear dispersion for the two cameras at different points throughout the part of the spectrum employed at times in planetary and stellar radial velocity determinations with the Lowell instrument follows.

TABLE III.

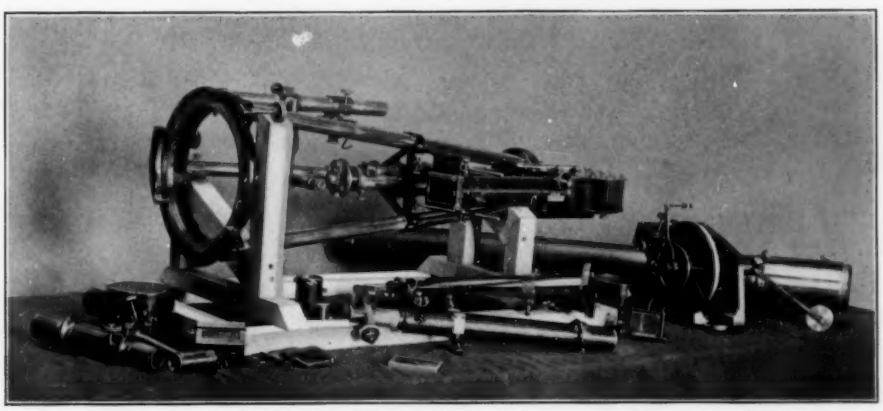
Line	Dispersion—Tenth-Meters per mm	
	Short Camera	Long Camera
$H\delta$	9.6	7.6
4200	11.4	9.1
4300	13.5	10.4
$H\gamma$	14.4	11.4
4400	15.7	12.4
4500	17.8	14.3

The limit of resolution of close lines on the sharpest negatives of

PLATE II.



A. The Spectrograph as Spectrometer.



B. The Spectrograph and Auxiliary Equipment.

the solar spectrum is about 0.11 tenth-meters at $H\delta$, 0.16 tenth-meters at λ 4300, and 0.22 tenth-meters at λ 4500. This is about the same as has been given for the other spectrographs. In the case of planetary and stellar spectrograms made on the most rapid plates—their grain being coarse—no such resolutions are possible; with such plates, and in the brightest stars, lines are not separated at $H\gamma$ which differ in wave-length by much less than 0.25 tenth-meters. In the case of faint stars, where the exposure time is longer and the slit-width greater, the practical resolving power is still less.

When it is desired to employ one of the single prisms or the grating the prism-box of the spectrograph is removed and the spectrometer section is mounted in its stead, as shown in Plate II A, which gives a view of the instrument arranged with the long-focus telescope for spectrometer work in the laboratory. The section is strongly constructed and carries a large position-circle accurately graduated to thirds of degrees and supplied with two verniers reading to half minutes. It has all necessary clamping and slow-motion screws. The single prism is mounted on the plate of the section by two screws which pass through the prism-mounting. The grating is similarly mounted. Metal boxes inclose the prism or grating.

If the single-prism equipment is to be used for photographing, the camera is inserted instead of the observing telescope, and the outer end of its tube is held by two rods which project from the web of the collimator section.

The grating is a Rowland plane grating of 14438 rulings to the inch.

There are two single prisms, one a very dense flint ,the other a light crown. Their angles, deviations, and indices of refraction are given below.

TABLE IV.

FLINT PRISM.

(Angle = 59° 59′ 50″.)

Line	Deviation	n
$H\delta$	60° 23′.8	1.7360
$H\gamma$	59 49.4	1.7310
$H\beta$	57 48 6	1.7131
D_2	55 50.8	1.6954
$H\alpha$	55 21.1	1.6905

CROWN PRISM.
(Angle = 59° 59′ 30″.)

$H\delta$	46° 58′.8	1.6076
$H\gamma$	46 37.2	1.6038
$H\beta$	45 50.0	1.5955
D_2	44 53 0	1.5855
$H\alpha$	44 30.7	1.5815

The dispersion at $H\gamma$ for the two cameras is, for the flint prism:

Long camera, 32 tenth-meters per mm,
Short camera, 39 tenth-meters per mm;

and for the crown prism:

Long camera, 62 tenth-meters per mm,
Short camera, 76 tenth-meters per mm.

It is possible, therefore, to select from the prism-train, these two single prisms, and the two cameras that dispersion which will deal most advantageously with the light of the star.

The microscope used in measuring the plates is modeled after the one described and illustrated in Frost's Scheiner's *Astronomical Spectroscopy*. As it was constructed with special reference to planetary work, it has a position-circle and a vernier reading to single minutes. Thus the planetary plates may be measured for displacement due to axial rotation either directly by finding the difference for settings on the two ends of the Fraunhofer lines corresponding to the ends of the planet's equator, or indirectly by measuring the inclinations of those lines. The indirect or inclination method has proved to be the more accurate and has been employed in all the measures for rotation.

This observatory stands 7250 feet (2210 m) above sea-level, where the barometric pressure is reduced by nearly one-fourth, so that there is a considerable decrease in the absorption by the Earth's atmosphere. This is a great gain in spectroscopic work of all kinds, but particularly so in planetary work, since it is necessary to observe some of the planets at low altitudes. Diminution in absorption, which shortens the exposure time, is equivalent to increasing the light-power of the equipment.

The following data got from the spectrogram record book will give an idea of the exposure times. The $H\gamma$ region of the spectrum

of *Venus* has been photographed on the most rapid plates in one minute with a slit-width 0.02 mm. Under the same conditions, plates have been made of *Jupiter* with exposures of 18 minutes; *Mars*, in about 25 minutes; and *Saturn*, in 2½ hours. The spectrum of *Sirius* photographs, under the best conditions, in 1 minute with a slit-width of 0.02 mm. Under the same conditions, *β Orionis* requires about 4 minutes; *Vega*, 5 minutes; *Arcturus*, about 8 minutes; *Aldebaran*, about 20 minutes; and *Polaris*, 30 minutes. With a slit-width of 0.02 mm, which is the width generally used, and under average conditions, a second-magnitude solar-type star is given an exposure of 40 or 45 minutes, and an *Orion* or Sirian type star of the same magnitude, about 15 minutes. These exposures are for the short camera. With the long camera they are increased by about one-fourth.

The comparison spectrum employed in the velocity work with both planets and stars has usually been that of the iron spark. The spectrum of the hydrogen tube has also been used in some cases for stars of the early spectral types. On the plates of the planets made for investigating the selective absorption due to their atmospheres, the spectrum of the Moon has, whenever possible, been photographed, because it offers a direct comparison between the planetary and the (normal) solar spectrum.

About fourteen hundred photographs of spectra have been made, mostly with the prism-train. Besides plates of artificial light-sources in the laboratory, this number includes negatives of the spectra of stars, Sun, Moon, and of all the planets of the solar system and of the asteroid *Vesta*.

The plates made in the laboratory and those of the Sun and Moon were principally for adjustment and test purposes, and for the reduction of the measurements of the planetary and stellar plates.

Although the spectrograph is devoted primarily to work on the planets, and stellar work is undertaken only as the time permits, about six hundred stellar spectrograms have already been obtained for the determination of velocities in the line of sight. These are mostly of southern stars—those not so convenient for observation at the more northern observatories. They are principally of stars of the earlier spectral types.

The spectra of the planets have been photographed from D to $H\delta$. Plates have been secured of all the planets except *Neptune* with the dispersion of the prism-train. The faintness of *Neptune* (8.5 magnitude) and the character of its spectrum make it a difficult object and permit the use of the dispersion of only a single prism. The plates of this planet are of more than ordinary interest on account of the light they throw on the nature and constitution of its atmosphere. The same may be said of the plates of the visual end of the spectrum of *Uranus*. The results of the investigation on the spectra of the planets, with reference to their atmospheres, will be published in due time.

Of the series of spectrograms made for investigating the rotation periods of the planets, the sets of *Venus* and *Mars* have been measured and the results published.[1] The set of *Mars* was made for the purpose of testing the efficiency of the spectrograph for investigating planetary rotation periods, and for this reason is referred to here. The velocity of a point on the equator of *Mars* due to axial rotation amounts to only 0.24 km per second. The slit of the spectrograph was placed upon the planet's equator, so that the observed spectrographic velocity would be four times the limb-velocity reduced for what the planet is out of opposition and for the tilt of the equator to the line of sight, or, in the present case, amounting to about 0.90 km per second. The displacements of the planetary lines were determined by measuring the amount they were inclined to the lines in the comparison spectra. Some of the plates were made with the camera above the collimator and some with it below, so that on some the lines are inclined to the right, on others to the left, under the microscope; and the measurements were made without knowing the position of the camera, and consequently without knowledge of the direction in which the lines should be inclined. Seven plates were so measured. The measurements gave the inclination in the proper direction for six out of the seven plates. The unweighted mean of all gave a point on the equator of *Mars* a velocity of 0.21 km, or 0.03 km too small; and a rotation period of 28.3 hours instead of 24.6 hours.

In this connection are given the measures and reduction of a plate of *Jupiter*. A direct enlargement from this spectrogram is reproduced in Plate III. The photograph was made November $21^d\ 16^h$

[1] *Lowell Observatory Bulletins* 3 and 4, and *Astronomische Nachrichten*, 3891–2.

G. M. T., 1903. The readings of the position angle of the microscope for the planetary lines and comparison lines are denoted by ϕ and ϕ_o, respectively. The spectrographic velocity

$$v_s = 4v\,(1 + \cos a)\cos B = V_s\, w\, D \tan \Delta\,\phi$$

where v is the limb-velocity, a the angle between the Sun and Earth seen from *Jupiter*, B the jovicentric latitude of the Earth, V_s the wave-frequency, w the computed width of spectrum on the plate, D the linear dispersion, and $\Delta\phi$ the inclination of the lines.

TABLE V.

Line	Position Angle for		Inclination of Planetary Lines	Dispersion Tenth-Meters per mm	Velocity of Limb	
	Planetary Lines	Comparison Lines				
λ	ϕ	ϕ_o	$\Delta\,\phi$	D	Observed	O.−C.
4171.1	9° 25′.0	.....	2° 19′.2	7.99	12.13	−.36
72.8	9 24.5	.....	2 18.7	8.02	12.12	−.37
73.6	9 28.0	.....	2 22.2	8.03	12.44	−.05
82.0	.....	7° 4.5	.....	....	....	....
87.3	9 23.0	7 6.0	2 17.2	8.23	12.26	−.23
88.0	9 24.0	7 6.0	2 18.2	8.24	12.36	−.13
99.3	9 26.0	7 7.0	2 20.2	8.40	12.76	+.27
4202.2	.....	7 5.0	.....	....	....	....
06.7	9 24.0	.....	2 18.2	8.51	12.72	+.23
15.6	9 17.5	.....	2 11.7	8.64	12.28	−.21
19.5	9 23.0	.....	2 17.2	8.70	12.87	+.38
22.4	9 23.0	.....	2 17.2	8.75	12.92	+.43
27.5	.....	7 4.5	.....	....	....	....
33.7	9 22.0	.....	2 16.2	8.92	13.06	+.57
35.4	9 16.5	.....	2 10.7	8.94	12.55	+.06
36.1	.....	7 6.0	.....	....	....	....
39.0	9 16.0	.....	2 10.2	9.00	12.57	+.08
40.0	9 13.0	.....	2 7.2	9.01	12.29	−.20
43.5	9 18.0	.....	2 12.7	9.07	12.89	+.40
45.5	9 15.5	.....	2 9.7	9.10	12.64	+.15
47.0	9 19.5	.....	2 13.7	9.12	13.06	+.57
50.3	9 16.5	7 7.0	2 10.7	9.17	12.82	+.33
51.0	9 9.5	7 8.0	2 3.7	9.18	12.15	−.34
54.5	9 10.0	.....	2 4.2	9.23	12.26	−.23
75.0	9 14.5	.....	2 8.7	9.53	13.08	+.59
82.6	.....	7 5.0	.....	....	....	....
93.3	9 5.0	.....	1 59.2	9.84	12.42	−.07
94.3	9 9.0	7 4.5	2 3.2	9.86	12.86	+.37
98.2	9 8.0	.....	2 2.2	9.92	12.83	+.34
99.4	.....	7 6.0	.....	....	....	....
4301.6	9 4.5	.....	1 58.7	9.97	12.52	+.03
07.0	9 10.5	.....	2 4.7	10.06	13.24	+.75
14.3	9 3.0	.....	1 57.2	10.19	12.58	+.09
15.2	8 59.5	7 5.5	1 53.7	10.19	12.21	−.28
4318.8	9 7.5	.....	2 1.7	10.26	13.14	+.65

Mean $\phi_o = 7°\ 5'.8$ Mean velocity of limb = 12.62 km

This plate gives the velocity of *Jupiter's* limb 0.13 km too large and the corresponding period of rotation ($9^h\ 50^m$) only five minutes too short.[1]

The results of the tests on the rotation of *Mars* and *Jupiter* give evidence of the satisfactory performance of the spectrograph in this line of work. A later paper will give some results of stellar radial velocity determinations made with this instrument.

LOWELL OBSERVATORY,
Flagstaff, Arizona,
April 1904.

[1] In computing the limb-velocity, the equatorial diameter of *Jupiter* was taken to be 141,940 km, and the mean period, $9^h\ 55^m$, was, by mistake, used. This period is, of course, not applicable to the planet's equator; for as A. Stanley Williams shows in *Monthly Notices*, **60**, 465, 1900, observers of *Jupiter* agree on a period of $9^h\ 50^m.4$ for the planet's equatorial zone. This period gives a limb-velocity of 12.63 km, with which the observed value 12.62 km is in excellent agreement.

PLATE III.

SPECTROGRAM OF *JUPITER* SHOWING ROTATION.

A DETAILED STUDY OF THE LINE SPECTRUM OF COPPER.

By A. S. KING.

IT is well known to every student of spectroscopy that the line spectrum of a metal is not invariable. It was early found necessary to distinguish between arc and spark spectra; and as our knowledge has advanced, it has become more generally recognized that a spectrum cannot be dealt with as a whole, but that each single line must be considered by itself. The same conditions of producing a spectrum may strengthen some lines, weaken others, broaden still others, etc., as compared with the spectrum produced by other conditions. So with copper we cannot speak of *the* line spectrum, but only of *a* spectrum which appears under certain given conditions. There is not only a spark spectrum and an arc spectrum, but an infinite variety of spectra, which often pass into one another by change of the manner of production.

Concerning the origin of vibrations, the mechanics of luminosity, we know as yet very little, in spite of numerous hypotheses. However, we have reason to hope that by close study of the conditions for the appearance of single lines we may arrive at some conclusions concerning these fundamental questions of spectroscopy. If we can show, for example, that some lines appear only with increased temperature, others with greater vapor density, and still others only when certain electrical oscillations are present, we shall have made a step in the desired direction. Clearly the problem is not to be solved by one person or in a few years. The conditions for the production of spectra are so extremely complicated, since by the change of one factor—as, for example, the current strength—so many other elements are at the same time altered that it is very difficult to refer an observed change in the spectrum to one definite cause. We can hope to grasp the true meaning of phenomena only through accumulated observations, given by different observers attacking the question from different points of view, and the investigation of numerous elements.

My study of the copper spectrum is thus to be regarded only as a beginning in this difficult field.

A detailed investigation of a single element is of importance in another direction. The statement that each line shows a special behavior does not apply to lines belonging to a series. Such lines are without doubt emitted by a single vibrating particle, and must therefore all change together. If, from among the lines not belonging to a known series, we can separate groups of lines which always change in the same way, we are justified in considering them as given by one and the same vibrating particle. Regularities in arrangement may then be looked for among the lines of such groups, and so the problem of luminosity is attacked from another side.

The study of all possible variations in the spectrum of a single element is a task of indefinite magnitude, and no completeness can be claimed for the work on the copper spectrum here presented; but the arc and spark spectra of copper have been studied as given by a large number of the most typical and diverse conditions, whose effects permit some conclusions to be drawn as to the way in which these conditions act.

Copper is a favorable element for an investigation of this kind on account of having numerous lines sensitive to change in physical conditions, and the arc between copper terminals can be controlled to a degree unusual among metallic arcs. The arc burns uniformly through a wide range of current, and without the violent combustion and rapid oxidation which with some metals prevent the accurate measurement of current and timing of exposures. With the spark spectrum, while here again the conditions of the spark can be readily controlled, a disturbing element is the presence of the strong air spectrum given by the condensed spark, which conceals a number of characteristic copper lines.

APPARATUS AND METHODS.

The voltage for the arc circuit gave a range from 36 to 440 volts by means of 90 storage cells and dynamo circuits of 220 and 440 volts.

For the spark, three induction coils were used, the largest being a Klingenfuss inductor giving a spark 1 m long, and allowing for a

variation of electromotive force by the use of different numbers of turns in the primary. Another coil gave a spark of 25 cm, but of considerable thickness; while a third coil gave but a 10 cm spark.

Leyden jars of 0.0027 henry each were used for capacity, and an inductance spool of 0.003 microfarad to observe the effect of self-induction. When gases were used around the spark, it was inclosed in a vertical glass tube through which the gases were passed, there being a side tube opposite the spark closed by a quartz window.

The spectrum was photographed by means of a concave grating of 1 m radius, adjusted to photograph the region from λ 2200 to λ 5800.

Besides the precautions necessary in manipulating the arc and spark to secure the desired conditions in each case, special care was required to avoid over-exposure, which tended to bring the weak and diffuse copper lines to the same intensity as the strong, sharp lines, these last being but slowly enhanced by longer exposures.

In comparing the intensities of lines, standard conditions were chosen for the arc and spark: for the arc a photograph taken with 220 volts and 5 amperes, and for the spark the spectrum as given by the spark in hydrogen, the air lines being absent. The intensities in each photograph were graded by means of a scale described in a previous article,[1] the appearance as well as the blackness of the lines being considered. The other photographs of arc and spark were th n compared line for line with these standard conditions, the scale being used to decide the amount of difference between corresponding lines.

The effort was made to secure comparison photographs of about the same average intensity. To equalize the effects of different exposure and development affecting the spectrum as a whole, it was found best to make one line of equal intensity for all the different conditions. In most of the photographs the line λ 4651 was but slightly changed, so after the intensities for each kind of arc or spark had been tabulated, as compared with the standard condition, this line was given the same value in each, and the values for the other lines in each case changed in the same proportion.

[1] A. S. KING, ASTROPHYSICAL JOURNAL, **19**, 225–238, May 1904.

The experiments with the arc included the use of voltages of 36, 72, 108, 180, 220, 440 volts, with a series of different current strengths for each, and also copper in the carbon arc. With 72 volts a maximum of 54 amperes was reached. With 180, 220, and 440 volts a current of 0.5 ampere would maintain a steady, though very small, arc; but lower than this the arc was constantly broken. For a still lower current, the arc was made in a succession of flashes, the arc breaking as soon as the terminals were separated, an hour or more of the constant interruptions being required to obtain a photograph. In these flashes the current was of course constantly changing, but did not rise above 0.3 ampere.

With the spark, a series of photographs was made with varying electromotive force and capacity, with the use of self-induction, hot electrodes, varying spark-length, and gap in series, as well as the spark in hydrogen and oxygen with and without an outside gap, and a study of the different regions of the spark as projected on the slit.

I give below only a few tables out of the material collected. These show some of the most diverse conditions of arc and spark, with the effects on the stronger copper lines. Besides these, I have made an extended comparison of the other arc and spark conditions, and the grouping of lines resulting from this will be treated in the discussion.

EXPLANATION OF TABLES.

Column 1: intensities of arc lines given by arc between copper rods, with dynamo current of 5 amperes and 220 volts.

Column 2: arc burning continuously with 0.5 ampere and 220 volts.

Column 3: interrupted arc of 0.3 ampere (maximum) and 440 volts.

Columns 4–7: spark spectra from large inductor with 10–12 amperes primary current and 4 mm spark-length. Column 4: condensed spark with 0.0135 microfarad capacity. Column 5: same capacity and with inductance of 0.003 henry in circuit. Column 6: with copper wires so thin as to glow and slowly melt, 0.0081 microfarad capacity being used. Column 7: spark in hydrogen with same capacity as (4) and (5).

The small dispersion renders it difficult to say much regarding the appearance of lines, and only some of the more notable characteristics are entered in the column of remarks. The mark (?) occurring frequently in the tables of spark spectra indicates that the intensity of the line is rendered uncertain by the superposed air or nitrogen spectrum. In the case of self-induction, such lines are in general weak, since they are concealed by the fine lines of the nitrogen bands. The Roman numerals refer to the grouping of spark lines which will be spoken of later.

Group	λ	1 5 Amp. 1	0.5 Amp. 2	0.3 Amp. 3	Condensed Spark 4	Self-Induction 5	Hot Electrodes 6	Hydrogen 7	Remarks.
I	2242.68	..	..	..	5	¾	3	3½	
I	2247.14	..	..	..	5	¾	3	3½	
I	2276.30	..	..	..	5	¾	3	3½	
	2293.92	2½	½	½	..	..	..	..	
I	2369.97	2	½	2	7	4	6	5	
	2392.71	2	½	0	..	..	..	..	
	2441.72	2	1	1	..	..	..	..	
III	2494.97	2	1½	1½	2½	3½	2	½	
I	2506.50	..	..	..	5	½	2	3½	
I	2529.60	..	..	..	2½	½	½	1½	
I	2545.08	..	..	..	7	1½	3	5	
I	2600.51	..	..	..	6	½	1	3½	
III	2618.46	6	4½	3	6	4	6	3½	
I	2666.61	..	..	..	2½	0	0	1½	
I	2689.66	..	..	..	7	¾	2	5	
I	2701.34	..	..	..	7	¾	2	5	
I	2703.48	..	..	..	5	½	1	3½	
I	2713.82	..	..	..	7	¾	2	5	
I	2719.14	..	..	..	5	½	1	3½	
I	2721.98	..	..	..	2½	0	½	1½	
III	2766.50	5	3½	2½	5	5	6	3½	
I	2769.88	..	..	..	10	1½	3	7	
III	2824.47	6	5	5	10	7	8	5	
I	2837.66	..	..	..	5	0	0	3½	
I	2878.02	..	..	..	5	½	1	3½	
III	2883.03	2½	2½	1½	2	3½	4	¾	
III	2961.25	7	6	6	10	8½	8	5	
III	2997.46	2½	2½	2	3½	3½	3	1½	
III	3010.92	4½	4½	4	7	6	5	3½	
III	3036.17	4½	4½	4	7	6	5	3½	
III	3063.50	4½	4½	4	7	6	5	3½	
	3073.89	2½	2½	2	..	..	..	..	
	3094.07	3	3	2½	..	..	..	..	
I	3099.97	2½	1	1½	6	?	3	3½	In (6) strongest at poles.
I	3108.64	4	1	2	8½	?	5	5	In (6) strongest at poles.
	3116.48	2	0	½	..	..	..	..	
I	3126.22	3½	1	2½	8½	?	5	5	In (6) strongest at poles.
I	3128.73	2½	0	1	3½	?	2	1½	
I	3140.42	2	0	1	2½	?	1	1½	
I	3142.47	3	½	1½	5	?	4	1½	
I	3146.93	2	½	1	2½	?	2	1½	
I	3169.73	2	½	1	3½	?	1	1½	
III	3194.17	4½	4½	4	7	6	5	3½	
II	3208.32	3½	3½	3	5	1½	3	1½	
I	3235.74	..	..	..	5	?	2	3½	
I	3243.21	..	..	..	7	?	4	5	
II	3247.65	40	34	28	27	21	27	16	Revised in (1).
II	3274.06	35	30	25	24	19	24	14	Revised in (1).
III	3279.89	3	2½	2½	5	?	4	3½	
I	3282.78	2	0	1	7	?	2	5	In (6) strongest at poles.
I	3290.62	3½	1½	2	10	3½	5	7	In (6) strongest at poles.

Group	λ	1 5 Amp.	2 0.5 Amp.	3 0.3 Amp.	4 Condensed Spark	5 Self-Induction	6 Hot Electrodes	7 Hydrogen	Remarks.
I	3308.10	5	3	4½	12	7	7	8½	In (6) strongest at poles.
I	3317.28	1½	1	1	6	?	2	3½	
III	3337.95	4½	5	3½	6	6	4	3½	
I	3349.38	1½	0	½	2½	?	1	1½	
I	3365.46	3	½	1	7	?	3	5	In (6) strongest at poles.
I	81.52	2	0	½	5	3½	1	3½	
	384.88	1	2½	2	..	..	..	..	
I	3450.47	3½	½	1½	8½	?	2	5	In (6) strongest at poles.
I	3454.76	2	2½	1	2½	?	½	1½	
I	3476.07	3	½	2	5	?	1	3½	In (6) strongest at poles.
I	3483.84	3½	½	2	8½	?	2	5	In (6) strongest at poles.
I	3512.19	4	1	2	5	?	3	4	In (6) strongest at poles.
I	3520.07	2	0	1	5	?	2	3½	
I	3524.31	3	½	2½	7	5	3	5	
I	3527.55	2	0	½	2½	?	2	3½	
III	3530.50	3½	4	3	5	?	4	1½	
I	3533.84	2	0	½	5	?	2	3½	
I	3599.20	5	2½	5	10	4	5	7	In (6) strongest at poles.
I	3602.11	5	2½	5	10	4	5	7	In (6) strongest at poles.
I	3613.86	2½	½	2	5	?	2	3½	
I	3621.33	2	0	1½	5	?	2	3½	
	3641.79	2	4	1	..	..	..	..	
	3654.60	4	2½	1	..	..	..	..	Broadened toward red.
	3684.75	3	5	2½	..	..	..	..	
I	3687.75	..	..	..	7	?	2	3½	
	3688.60	7	3½	1½	..	..	..	..	Broadened toward red.
	3700.63	2	0	1	..	..	..	..	
	3741.32	2	0	1½	..	..	..	..	
	771.96	1½	0	½	..	..	..	..	
	3805.33	1½	0	1½	.	..	..	..	
	3821.01	2	?	1½	..	..	..	..	
	3825.13	3	1½	1	..	..	..	..	
III	3860.64	3½	3½	3	2½	?	4	3½	
	3861.88	4	2	2	..	..	..	..	
	4015.80	1½	½	1	..	..	..	..	
III	4022.83	13	10	10	10	8½	14	7	Broadened toward red.
I	4043.70	..	..	..	?	3½	0	5	
	4056.80		8	4	..	..	..	..	
II	4062.94	15	12	12	12	10	18	8½	Broadened toward red.
	4123.33	2½	0	1	..	..	..	..	Diffuse
II	4177.87	4	½	2	?	3½	4	3½	Diffuse.
	4242.42	2	0	1	..	..	..	..	Diffuse.
III	4249.21	5	3	6	5	5	6	5	
	4259.63	4	½	3	..	..	..	..	Diffuse.
II	4275.32	10	9	12	15	8	14	10	
II	4378.40	9	3	8	10	6	10	7	Diffuse. In (5) strongest at poles.
I	4415.79	5	1	4	10	3½	4	3½	Diffuse. In (5) strongest at poles.
III	4480.59	8	8	8	5	5	6	3½	
I	4507.62	6	0	4	8	2	3	4	Diffuse. In (5)and (6) strongest at poles.
II	4509 60	7	6	10	10	5	7	7	In (5) and (6) strongest at poles.

Group	λ	1 5 Amp. 1	0.5 Amp. 2	0.3 Amp. 3	Condensed Spark 4	Self-Induction 5	Hot Electrodes 6	Hydrogen 7	Remarks
III	4531.04	9	9	9	?	8½	8	5	
II	4539.68	8	1½	6	7	3½	6	7½	Diffuse. In (6) strongest at poles.
I	4555.94	..	..	..	5	1½	1	3½	
II	4587.19	9	3	8	10	5	8	8½	Diffuse. In (5) and (6) strongest at poles.
	4642.78	3	0	2	..	..	..	..	Diffuse.
III	4651.31	12	12	12	12	12	12	12	
	4674.94	5	½	3	..	..	..	..	Diffuse.
	4697.62	3	0	2	..	..	..	..	Diffuse.
III	4704.70	7	7	7	?	5	4	5	In (5) and (6) strongest at poles.
-	5076.42	2	1	¼	..	..	..	..	
II	5105.75	17	18	17	19	14	18	10	
II	5153.33	16	14	16	19	12	16	14	
II	5218.45	18	16	18	24	15	20	17	
II	5220.25	6	7	7	10	8½	8	7	
II	5292.75	7	6	9	12	7	6	7	In (5) and (6) strongest at poles.
	5391.89	3	0	1	..	..	..	..	Diffuse.
	5432.30	2	0	½	..	..	..	..	Diffuse.
	5536.06	3	0	1½	..	..	..	..	Diffuse.
	5555.16	2	1	3	..	..	..	..	
III	5646.93	6	5	4	10	8	8	5	
III	5700.39	12	13	12	13	12	14	6	
III	5782.30	15	16	15	17	15	18	8½	

THE ARC SPECTRUM.

1. *Effects of varying current.*—Comparing the three arc spectra tabulated, which I shall designate respectively as the strong, weak, and interrupted arcs, we can see how far the conclusions of Hartmann[1] hold for the copper spectrum. Hartmann found that in the spectra of magnesium, zinc, bismuth, and lead certain strong spark lines which are absent or very weak in the strong arc appear with considerable intensity in the arc with very small current, constantly broken. This result seemed to depend primarily upon the current being continually interrupted, which would be expected to produce strong oscillations in the arc, and to approach as near the spark conditions as the low electromotive force would allow.

The copper spectrum contains no lines on which the action of the weak arc is so decided as on the line λ 4481 of magnesium, for example. The strong arc gives distinctly all lines which appear in the arc under any circumstances. The interrupted arc does not

[1] ASTROPHYSICAL JOURNAL, **17**, 270, 1903.

bring out any lines which usually appear in the spark alone. There are, however, a number of lines in the spark spectrum which are greatly weakened by self-induction, hot electrodes, and other conditions which weaken the spark discharge. These may then be designated as "spark lines" in the sense that they are favored by the conditions of the strong spark. It will be seen from the tables that, as a rule, when these lines appear in the arc at all, they are much reduced by the weak continuous arc, but *restored to considerable intensity by the interrupted arc*, sometimes becoming relatively stronger than in the strong arc. Types of this class of lines are the following:

3108	3308	3484	3614
3126	3450	3599	3621
3290	3476	3602	4416

The evidence seems strong that the interruptions are responsible for this change. The intensities of these lines suffer, as it were, a discontinuity, the lines becoming gradually weaker with the decrease in current until the arc will no longer burn continuously; then with a current only slightly weaker, the lines suddenly regain a considerable relative intensity. They are for the most part diffuse lines, and their decrease in intensity would be explained by the reduced vapor density of the weak continuous arc, were it not that we should expect a still smaller vapor density in the interrupted arc. Of this last point, however, we cannot be certain. It is conceivable that the high voltage producing a flash which is at once extinguished may give a high vapor density for this very brief time, higher than the very small arc burning quietly with a slightly greater current. A reduced vapor density probably exists in those spark conditions which weaken these lines, and they are relatively enhanced by increased capacity, which, while it strengthens the electrical discharge, must increase the vapor density.

It would thus seem that we must admit the possibility of the dependence of this set of lines on vapor density, with the temperature changes which would accompany change of vapor density. It seems to the writer more reasonable, however, to attribute the lines to oscillations in the arc and spark—oscillations not necessarily so violent as those which excite the vibrations of the distinctly spark lines

which never appear in the arc. Oscillations are doubtless present to a greater or less extent in the strong arc, and are favored by its more or less irregular burning and the fact that inductance is present with a dynamo circuit, and capacity with a battery of storage cells. These oscillations are probably much reduced by the weak continuous arc, which burns more steadily and usually very silently. The interrupted arc, being constantly rekindled, should greatly favor the oscillations. In the spark spectrum the behavior of these lines bears out the view that they are dependent on the strength of the oscillations. They are much reduced by self-induction in the spark circuit, and slightly enhanced by increased capacity. In a long and powerful spark they are much weaker in the middle of the spark than close to the electrodes. Also in the spark between glowing electrodes, in which experiments indicate that the oscillations are much weaker, we find these lines reduced.

The arc lines in my table are those appearing with moderate current strength. With a current of 8 to 10 amperes a large number of diffuse lines appear, chiefly in the blue and green, and become rapidly stronger as the current is increased, their vibrations probably requiring the stimulus of the high vaporization. The arc lines given by Kayser and Runge[1] and not in my table belong to this class.

2. *Altered voltage and conductivity.*—While with the copper arc the variation of current strength proved to be the most effective way of altering the spectrum, other methods produced changes which will now be noticed. The best type of a change by altered voltage was given by comparing the spectrum with 1 ampere and 440 volts dynamo current with that given by a storage battery current of the same strength with 72 volts. A number of lines were changed relatively to other lines. Considering a few distinctive cases, the pairs λλ 3654, 3688, 4480, 4531 were enhanced by 440 volts relatively to the lines λλ 3684, 4509 adjacent to each pair. These pairs are changed in the same way by the use of inductive resistance in the otherwise non-inductive circuit of 90 storage cells, with 0.5 ampere and in addition we have now the pairs λλ 4023, 4063, 5153, 5218 5700, 5782 strengthened relatively to the adjacent lines λλ 4056,

[1] Anhang zu *Abhandlungen der Kgl. Akad. der Wiss. zu Berlin*, 1892.

5105, 5646. The changes are slight and may be the result of oscillations given by the inductance of the dynamo circuit and by the storage battery circuit with inductive resistance, as compared with the non-inductive circuits. These pairs are among the most distinctive arc lines. The "spark lines" considered above are too faint in these weak current arcs for comparison.

Copper in the carbon arc gives an arc of different conductivity from that between copper rods, because of the division of the current between carbon and metallic particles. A number of copper lines are much weakened, compared with an arc of the same current between copper terminals, chief among these being λλ 3365, 3381, 3450, 3483, 3599, 3602, 3613, 3621, 3654, 3688, 4023, 4063, 4249, 4378. These lines are in all cases shown by my table to be much reduced by the weak continuous arc, and generally restored to considerable intensity by the interrupted arc. The action of the copper-carbon arc suggests an explanation of this. In the first place, the density of copper vapor is probably much less than in the copper arc; and, secondly, the copper-carbon arc burns much more quietly and uniformly than the copper arc of equal current, suggesting a weakness of the oscillations which was noted as a probable property of the low current copper arc.

Nitrogen bands in the weak arc.—It has been several times noted in recent years that the negative-pole nitrogen bands sometimes appear in the carbon arc. In the copper arc spectrum, as given by 440 volts and 0.5 ampere, I have obtained very distinct nitrogen bands, not only the heads, but the whole structure of the bands, being clearly visible, almost as strong as in the spark spectrum with self-induction. The bands were visible, but not so strong with the weak interrupted arc of 440 volts. The conduction of a considerable part of the current by the nitrogen would be expected to weaken some lines more than others, and this division of the current may play some part in producing the changes observed with the weak arc. As regards the medium between the electrodes, the arc with weak current and high voltage thus appears to approach the spark discharge.

3. *Different regions of the arc.*—An important change was observed when the outer part of the middle of a long arc was projected on the

slit, and this photograph compared with one taken with the same current and same length of arc with the part next to the positive pole projected. A much longer exposure was needed for the outside of the arc, and the result gave a group of lines much reduced relatively to the others. When the lines λλ 3247, 3274, 3337, 3524, 5105, 5646, 5700, 5782 and the strong arc lines of shorter wave-length are brought to nearly equal intensities in the photographs by properly timed exposures, a large group of characteristic lines is much reduced. These lines are as follows:

3308	3688	4480	4704
3599	4023	4509	5153
3602	4063	4531	5218
3654	4275	4651	5220

In this latter group the line pairs λλ 3654, 3688, 4023, 4063, 4480, 4531, 5153, 5218 are reduced somewhat less than the strong unrelated lines λλ 4275, 4378, 4509, 4651, 4704.

The experiment corresponds to Lockyer's method of long and short lines with a prismatic spectrum.[1] It serves to separate the lines present in all parts of the arc from those given strongest by the central portion, where temperature and vapor density are at a maximum and the vapor purest. However, that temperature or vapor density is the controlling element for these lines is rendered doubtful by the fact that some of the lines most reduced by the outer layer of the arc, viz., λλ 4275, 4651, 4704, are reduced little, if at all, by the arc with very weak current. Lecoq observed them in a weak spark between electrodes containing copper chloride. Also, λ 5218, which is considerably reduced in the outer layer, appears in the Bunsen flame. The most plausible explanation seems to be that these lines are favored by (though not dependent upon) strong oscillations in the arc. These oscillations should be much weaker in the outside layer than in the middle next the pole. The facts that the lines are often strengthened in the interrupted arc, and in the spark are generally reduced by self-induction, accord with this view. Furthermore, the group which appear strong in the outside layer of the arc, and are to be regarded as the "persistent" lines of the spectrum, are as a rule little changed by altered spark discharge, especially the

[1] KAYSER, *Handbuch der Spectroscopie*, **2**, p. 236.

strong arc lines below λ 3247, which show the least change in the outside layer of the arc.

De Watteville[1] has recently shown that with potassium in the coal-gas flame a series of zones exists, each of which gives off lines belonging to a particular series. So far as the known series relations allow, the copper lines show a similar behavior in the different parts of the arc: the strong ultra-violet pair appearing in the outermost layer of vapor, the pairs of the two subseries being then somewhat reduced, but not so much as the majority of the lines, which belong to the central part of the arc.

4. *Further notes on the arc.*—*a*) With a current slightly above 1 ampere, the relative intensity of lines is *independent of voltage.* Batteries of 36, 72, 108, and 180 volts were used, also dynamo voltages of 220 and 440, and were found to give identical spectra for the same current strength, except with very weak currents.

b) With a continuous arc the changes given by varying current are *always gradual.* This was determined by a series of photographs with different currents at each voltage. As an example, with low current λ 5153 is much weaker than λ 5105; the lines become equal at about 6 amperes, and λ 5153 becomes gradually stronger as the current rises.

c) *A change in length of arc,* with the same voltage, alters the spectrum only by changing the current. For example, the current may be reduced from 5 to 3 amperes by increasing the length of arc, and the relative intensities of some lines are thereby altered. The spectrum in the latter case, however, was found to be the same as that with the short arc when the current was reduced to 3 amperes by outside resistance.

THE SPARK SPECTRUM.

1. *Changes by altered discharge.*—The changes in the spark most effective in producing relative differences among the copper lines were found to be the introduction of self-induction, the use of electrodes so thin as to become glowing, the use of atmospheres other than air, and the comparison of the middle of a long and thick spark with the part next to the electrode. Surprisingly small differences were given

[1] *Comptes Rendus,* **138**, 346, 1904.

by a large change of capacity or of electromotive force. It appeared that a small capacity, with an inductor giving a spark only a few centimeters long, was sufficient to produce the regular spark spectrum; and a large battery of Leyden jars with a powerful induction coil, while greatly shortening the exposure required, produced only a slight intensification of those lines most characteristic of the condensed spark. A radical change in the character rather than in the strength of the discharge was required.

The first important result to be noted is the similarity of the spectra given by the use of self-induction, by hot electrodes, and by the middle of a long spark. While these spectra are not identical, a larger number of lines, especialiy in the ultra-violet, are much weakened in all three cases. These lines are given of maximum intensity by large capacity, and when the end of a long and powerful spark from the large inductor is projected on the slit, the middle of this same spark showing the lines very weak. This combination of effects seems to justify ascribing these lines to the oscillations in the spark. Those which are chiefly affected are the lines most characteristic of the condensed spark, most of them never appearing in the arc. The group (see table) from λ 2689 to λ 2719 are types of this class. The reduction of these lines makes the spark spectrum more like that of the arc, which in other metals has often been observed to be the effect given by self-induction and by glowing electrodes.

In my tabulation I have divided the spark lines of copper according to their behavior under various conditions into three groups, as indicated by the Roman numeral opposite each line. In Group I are placed those lines which occur only in the spark, and also some lines the behavior of which, in the spark, while they appear in the arc, indicates that they are especially favored by the conditions of the strong spark. The lines of this latter class are enhanced when a more powerful spark is used, or when the part of the spark next the electrode is projected on the slit. They are much reduced by self-induction, by hot electrodes, and usually by atmospheres of hydrogen and oxygen. With self-induction and hot electrodes they show stronger at the poles, while the more persistent lines extend uniformly across the spark. Unlike the lines which appear in the spark alone, these lines are given distinctly by the middle of the

long and thick spark. This peculiarity, and the fact that they occur in the arc, points to the conclusion that, while they are favored by strong oscillations, the lack of these may be made up for by a strong vaporization, such as is present in the thick spark, and (compared to the spark) in any kind of arc. In the arc, the tables show these lines to be almost without exception greatly reduced by the small continuous arc, and restored to almost, if not to full value, by the interrupted arc. It is in this sense, as has been noted, that the interrupted arc with very weak current strengthens the spark lines.

Groups II and III consist of lines common to arc and spark, but which are not easily assignable to any one condition as sole cause. The lines of Group II are much reduced by self-induction, but their behavior under other conditions does not justify placing them in Group I. They are reduced in most cases by the weak continuous arc and appear with considerable strength in the spark with hot electrodes, when there is probably strong vaporization. These facts point to vapor density as governing the intensity of these lines, but their origin is evidently complex. The pair λλ 3247, 3274 are in all cases the strongest lines of the spectrum, and are the only lines which I have been able to obtain with an uncondensed spark. They were noted by Hartley[1] with a 0.01 per cent. solution of copper salt in the spark.

Group III includes the remaining lines in the table and are little altered by self-induction, hot electrodes, or the middle of a long spark. They are also the lines least changed by the three arc conditions tabulated. In the spark they are quite generally reduced by hydrogen and oxygen.

The effect of a *spark-gap in series* was tried with the spark in air, hydrogen, and oxygen. The greater part of the energy being used in the long outside gap, the smaller spark was reduced in intensity; but when this was compensated for by longer exposures, the chief difference which appeared was a relative strengthening of the true spark lines which are absent in the arc. This should result from the greater violence of the discharge given with the long auxiliary spark.

Small changes in *length of spark* produced little or no relative

[1] *Phil. Trans.*, **175**, II, 325, 1884.

change in the spectral lines. With a spark-length of 1 cm or more, however, as noted before, the lines most characteristic of the spark appeared much stronger near the electrodes than in the middle of the gap.

A comparison of the *different regions* of the spark at the same distance from the electrodes was made by projecting in the one case the center of the luminous track, and in the other case the outer green coating of the spark on the slit. The latter photograph required three times as long an exposure as the first for the same intensity of the copper spectrum. The air spectrum was then almost entirely eliminated, showing that it belongs entirely to the luminous track, but surprisingly few relative changes appeared among the copper lines. The pair λλ 4023, 4063 are considerably strengthened in the outer portion of the spark, and other arc lines show slight changes. The experiment seems to afford strong evidence that the electrical action, rather than temperature or vapor density, is the governing element in the production of the spark spectrum. The electrical conditions should be nearly the same in the spark at the same distance from the electrodes, whether in the middle or outside of the spark, while the outer layer should have a lower temperature. The differences in the arc lines and the still larger changes previously noted when different regions of the arc were used may be explained if the arc lines are more dependent on temperature and vapor density, the properties in which the zones probably differ.

2. *Effects of atmospheres.*—The numerous changes in the character of the discharges by altered circuit conditions have in no case given a spectrum similar to that given by the spark in an atmosphere of hydrogen or oxygen. On this account it appears that the cause of the action of various gases is not to be looked for in changes of electromotive force, strength of discharges, period of oscillation, or temperature; but rather by the altered conductivity afforded by each gas. This would result in an altered division of the current between gas and metallic particles, and in this re-division it is highly probable that the electric stimulus needed for certain vibrations would be strengthened, and that for others weakened. The tabulation shows the numerous differences between the copper spectrum in hydrogen and in air. A large part of the current was evidently carried by the

gas, for although the bright red spark was as thick as in air, nearly ten times as long an exposure was required to bring the copper lines to intensity suitable for comparison. The hydrogen lines were very strong, $H\beta$ being reversed.

An atmosphere of pure oxygen, while not requiring so long an exposure as hydrogen, gave a very similar spectrum, but with relative differences in several important lines. With most of the lines of the same intensity with hydrogen and oxygen, λλ 4249, 4275, 4416 were given stronger in oxygen, and λλ 2618, 2766, 2837 were weakened. The oxygen spectrum was strong.

Naturally this altered conductivity is accompanied by an altered potential, but the changes given by a change of the conducting gas between the electrodes are different from the changes given by altered potential with the same gas.

A comparison of my table of intensities of the copper spark in hydrogen with that of Eder and Valenta[1] shows some relative differences between lines too large to be explained by different standards of intensity. For instance, the lines λλ 4275, 4378, given in my table as 10 and 7 respectively, are given by Eder and Valenta as 10 and 1. Also several lines, chiefly in the ultra-violet, which are very weak or absent in my photographs, are given by Eder and Valenta as of considerable intensity. This lack of agreement is doubtless largely due to the difference in the inductors used, and illustrates the difficulty of comparing intensity tables of spark spectra made by different observers by means of different apparatus. Further, it is quite possible that hydrogen, by reason of carrying so large a proportion of the current itself, makes the copper vapor more sensitive to changes in the character of the discharge, since I found that in air the copper lines are changed but slightly by the use of different inductors and capacities.

As another instance of the differences in spark spectra given by different kinds of discharge, I may cite the results of Demarçay[2] with copper-chloride solution. With his inductor having a secondary coil of thick wire, Demarçay obtains a spectrum greatly different from the usual spectrum of the spark in air, and approaching the arc spectrum, as indicated by the weakness of λ 5105 with respect to λ 4651, and the strength of the pair λλ 4023, 4063.

[1] *Denkschriften der Wiener Akad.*, **63**, 1896. [2] *Spectres électriques*, 1895.

It was expected that the introduction of a long outside spark-gap would reduce the differences given by hydrogen and oxygen around the short spark which was photographed, as the circuit conditions would be governed largely by the long gap in air which remained unchanged. The effects of the gases were not materially altered by this means, however, the only effect being a relative enhancement of some of the more distinctive spark lines, as was the case when an outside gap was used with the spark in air. This afforded additional evidence that it was the re-division of electricity between the two carriers, rather than an altered character of discharge, which produced the observed changes.

3. *The spark in water.*—Dr. Konen[1] found the arc spectrum of copper in water to be little changed from that in air. He has kindly furnished me with a photograph of the spectrum of the copper spark in water, taken in the course of his investigation. The strong continuous ground does not permit an accurate estimate of the intensities of the copper lines; but, so far as can be judged, the spectrum is but slightly changed from that of the condensed spark in air, except for the broad reversal of λλ 3247, 3274. One noteworthy difference is the weakness of λ 5105 compared to λ 5153. This appeared only in the spark spectra I have studied with atmospheres of hydrogen and oxygen, most decided in the case of hydrogen. This points to the hydrogen liberated by electrolysis as the cause of the slight change noted with the spark in water, which is in agreement with the conclusion of Hartmann and Eberhard,[2] that hydrogen is responsible for the alteration of arc spectra by water.

4. *Regularities and general structure of spectrum.*—In regard to the series relations, the rapid decrease in intensity of the successive members of the first subseries,[3] which in the arc spectrum renders the ultra-violet members invisible, is still more pronounced in the spark, the pair λλ 3654, 3688 not appearing, and the difference between the pairs λλ 4023, 4063 and λλ 5153, 5218 being greater than in the arc. In fact, the difference in intensity between corresponding lines of these pairs, as λλ 4023 and 5153, is a good indica-

[1] *Ann. der Phys.*, (4) **9**, 742, 1902.

[2] ASTROPHYSICAL JOURNAL, **17**, 229, 1903.

[3] See KAYSER, *Handbuch der Spectroscopie*, **2**, 530.

tor of the strength of the spark conditions, the difference being small with self-induction and hot electrodes, and very large next to the electrode with a long spark.

In addition to the known series, line pairs may frequently be selected which are changed in the same way by changed conditions. The arc pair λλ 4242.78, 4697.62, having approximately the same vibration-difference (251.4) as the series pairs, appears not to have been noted before. Some of the lines differing by numbers which Rydberg[1] found to be often repeated show similar behavior, but in many cases the different behavior of such lines indicates that they cannot be considered as *pairs* in the usual sense. As examples of such couples whose lines do not change in the same way, we have λλ 3126, 3194 and λλ 3063, 3129, each with the difference 681; also λλ 4651, 4697 and λλ 3614, 3642 with the difference 212, and others of the same sort. This dissimilar behavior, and the fact that a line was often found by Rydberg to be coupled with two or even three other lines by these frequently occurring differences, suggest that the differences arise from relations connecting the mathematical expressions, probably complex, for the vibrations of separate particles, rather than the grouping of lines which belong to the same particle.

Considering for a moment the copper spectrum as a whole, we see that strong spark discharges result in enriching the ultra-violet, the lines depending most on strength of discharge being in this region. The extreme arc conditions, given by a high current between copper rods, have their chief effect in the green and yellow, bringing out a large number of diffuse lines, which seem to require high vapor density. This relation between the effects of the two extreme conditions suggests a dependence on the principle, which is general in vibrating systems, that the vibrations of shorter period require a more violent stimulus—a stimulus which in this case is given by the powerful oscillations of the condensed spark.

CONCLUDING REMARKS.

The investigations of Schuster and Hemsalech[2] on the electrical processes in the spark gave us a picture of the manner of production

[1] ASTROPHYSICAL JOURNAL, **6**, 239, 1897.

[2] *Phil. Trans.*, **193**, A 189, 1889; also G. A. HEMSALECH, *Thèses*, Paris, 1901.

of the spectrum: the first discharge passing through the air, and the succeeding oscillations through the metallic vapor, the alterations in these oscillations, especially by the use of self-induction, changing the spectrum, enhancing some lines at the expense of others. Following this, the work of Schenck[1] showed that the spectral lines are dependent to different degrees on the oscillations, each line by his method showing the oscillations which produced it, and how far the intensity of the line was due to the electrical pulses and how far to the luminous vapor in the middle of the spark. His results showed that the distinctly spark lines give the oscillations much stronger than the lines which appear also in the arc. The action of self-induction in altering the period and character of the oscillations thus explains the fact that the lines most peculiar to the spark are most sensitive to the effects of self-induction, a reduction of these spark lines making the spark spectrum more like that of the arc, the arc lines depending least on the oscillations. Schenck and other observers have noted that glowing electrodes have an effect on the spark lines similar to that of self-induction. The heating of the medium makes it better conducting, and the electrical pulses are weakened. But with this increased conductivity comes a reduced potential, and probably considerable changes in temperature and vapor density. The change in division of the current between metallic particles and the gas between the electrodes, to which reference was made in the case of change of atmosphere, may have a considerable part in the changes given by self-induction and by hot electrodes. The spark between copper electrodes in air gives the air lines strong. This means that a large part of the energy of each spark is used up in producing the air spectrum and is not available to act on the copper vapor. With hot electrodes the air spectrum is much weakened, and the nitrogen bands appear faintly; while with self-induction the air lines have given way entirely to a strong nitrogen spectrum, doubtless in each case with a resulting change in the division of energy between metallic particles and gas. In the middle of a long spark we have a reduction in the strength of the oscillations, but a strong air spectrum and considerable vapor density.

My photographs show the copper spectra with self-induction,

[1] Astrophysical Journal, **14**, 116, 1901.

with hot electrodes, and in the middle of a long spark to be by no means identical, though having the common property of a reduction of the characteristic spark lines. The oscillations of the spark are weakened in each case, but the three conditions differ among themselves sufficiently to account for the differences in lines which probably do not owe their origin to any single state of the luminous vapor.

This work has been largely a study of the relations of various electrical conditions. The results for the spark spectrum show that the usual spark spectrum is given by a condensed spark of moderate intensity, and is very sensitive to changes which modify the character of the oscillations. As with other elements which have been studied from this point of view, a group of lines, chiefly in the ultra-violet, may be selected which appear to depend chiefly on the strength and frequency of the oscillations. Other lines depend to a less degree on electrical action, and others are of a still more complex origin. The chief changes in the arc spectrum were obtained by a variation of current strength, and by the weak interrupted arc. While the spectral changes in the arc can with some difficulty be explained by changes of vapor density, a comparison with the spark spectrum under various conditions, and a consideration of the oscillations in the arc, strong with high current, reduced by the weak continuous arc and given again strongly by the interrupted arc, appear to justify the conclusion that in the arc as well as in the spark the relative strength of a large number of copper lines depends upon the character of the oscillations present.

My hearty thanks are due to Professor Kayser for his constant interest, and for much advice given during the investigation.

UNIVERSITY OF BONN,
April 1904.

THE CORRECTION OF THE STANDARDS OF WAVE-LENGTHS.

By J. HARTMANN.

IN an earlier paper[1] I have pointed out the errors of Rowland's system of wave-lengths and their origin. The conclusions which I there drew have been meanwhile fully confirmed by Messrs. Perot and Fabry[2] and by Kayser.[3] The only difference of opinion which still exists refers to the manner which is to be adopted for creating reliable standards of wave-lengths. Although it is admitted both by Kayser and by Perot and Fabry that for all spectroscopic investigations the *absolute* value of the wave-lengths is a matter of entire indifference, they nevertheless come to the conclusion that simultaneously with the correction of the relative wave-lengths the transfer should be made from the system of Rowland to the absolute system of Michelson. Inasmuch as great confusion would be introduced in all statements as to wave-lengths by the very considerable change of the values of all wave-lengths involved in this operation, and inasmuch as the utilization of all measurements up to this time would be rendered much more difficult, I have proposed that this entirely useless change should not be made, and at the same time I suggested the very simple way of obtaining a new and wholly correct system of wave-lengths which fitted Rowland's as closely as possible. That feature of my proposal which is opposed by Kayser depends for the most part on an incorrect understanding of my article, and in view of the importance of the matter I regard it as desirable that I should once more clearly set forth the relations of things.

The demands to be made of a system of wave-lengths are wholly different according to the object for which they are to be employed. If a system of standard lines is principally to serve for furnishing the

[1] ASTROPHYSICAL JOURNAL, **18**, 167, 1903; *Zeitschrift für wissenschaftliche Photographie*, **1**, 215, 1903.

[2] ASTROPHYSICAL JOURNAL, **19**, 119, 1904; *Ann. de Chim.*, (8) **1**, 5, 1904.

[3] ASTROPHYSICAL JOURNAL, **19**, 157, 1904; *Zeitschrift für wissenschaftliche Photographie*, **2**, 49, 1904.

fundamental net for laboratory determinations of wave-lengths, from which all the other wave-lengths are to be determined by interpolation, then the principal issue is that the errors of this system of standards shall have a *regular* course, and that individual lines shall not show large random deviations. For if the latter were the case, as Kayser rightly points out, very different wave-lengths would result for the dependent lines according to the choice of a standard. Hence for the purpose named it would not be important to have a system of rigorously correct *relative* wave-lengths, but only that its errors should have such a continuous progression within long stretches, that the interpolation should at no point be rendered difficult.

A second requirement for perfectly correct *relative* wave-lengths is made on the system of standards in the case of all those applications where widely separated portions of the spectrum are brought into mathematical relations to each other. This is the case for interference methods, for observations of coincidences with gratings, and for all investigations as to the series structure of the lines.

The third requirement, that the *absolute* values of the wave-lengths should also be correct, arises only in those cases where the wave-length of light is to serve as the measure for other magnitudes. In all these purely metrological operations the case is, however, simplified by the fact that the observer always makes use of certain definite and entirely distinct kinds of rays, as the cadmium lines, the wave-lengths of which can be adopted according to what seems to be the most correct values obtained up to the time they are required.

If we keep the actual practical necessities before our eyes in this way, we shall obtain the following succession of operations which must be executed, arranged in order of their urgency:

The first and most urgent problem is the development of a system of *uniform* standards extending over the whole spectrum; that is, a system all the lines of which should be so fully assured by careful measurements on numerous good grating negatives that the accidental errors of the individual lines should not exceed the value of 0.003 tenth-meter.

In my previous essay I showed first how, by the application of empirical corrections as well as by the employment of plates of very different quality for the separate lines, Rowland introduced errors in

his standards in the solar spectrum and in his standards in the arc spectrum, which were distributed altogether irregularly and are now wholly beyond checking. I therefore expressly emphasized the fact that *for just this reason all of Rowland's standards are not to be used as a basis for accurate measurements.* I further showed that for our purpose there are now only two systems which can come into consideration, namely: first, the wave-lengths from the *Preliminary Table of Solar Spectrum Wave-Lengths* (which I designated by P. T.), and, secondly, Kayser's standards from the arc spectrum of iron, which I designated by K.

Thanks to the excellent plates of Rowland and the careful measures of them by Jewell, the values of the P. T. are to be regarded as a perfectly *uniform* system, satisfying the first of the requirements stated. But indispensable as the use of this system is for the astrophysicist—particularly in the measurement of stellar spectra of Types II and III—the use of the solar spectrum for a comparison spectrum for photographs taken in the laboratory commends itself very little to the physicist. Hence, with entire propriety, Kayser's iron spectrum has been generally adopted as the comparison spectrum for all determinations of wave-lengths of terrestrial sources of light. It is, indeed, dependent upon the *irregular* system of Rowland's standards in the arc spectrum of iron, but by his careful measurements of numerous grating plates Kayser has smoothed out as far as possible the irregularities of the standards, so that his standards from the arc spectrum of iron may also be regarded as a sufficiently uniform system. My proposals refer essentially to the use of these two *uniform* systems, whence may be recognized the lack of foundation of Kayser's objection[1] that the adoption of my suggestion would leave certain larger errors unchanged.

Kayser's standards unfortunately end at λ 4495, and therefore we must regard a uniform continuation of the iron standards through the whole visual spectrum as the most important and urgent problem. Hitherto the observer has always served his purpose by using Kayser's values for wave-lengths shorter than λ 4500, but for longer wave-lengths has simply adopted the values of the P. T. for the solar spectrum in p ace of the wave-lengths of the arc lines of iron. This

[1] *Zeitschrift für wissenschaftliche Photographie*, **2**, 53, 1904.

procedure is wholly unpermissible, as the positions of the lines in the arc spectrum deviate in an irregular manner from those in the solar spectrum. If it was desired to establish a direct connection with the values of the P. T., the solar spectrum should be photographed as a direct comparison spectrum, which presupposes an experienced observer and a very perfect adjustment of the spectrograph, as can now be attained in astrophysical apparatus.

The unavoidable coexistence, both present and for the future, of the *two* systems of standards now leads us to the problem of accurately determining their relation, which is of first importance for astrophysics. Designating by λ_P the wave-length of any given line measured precisely on the system of the P. T., and by λ_K the wave-length of the same line referred to the system of K., I placed in my former paper

$$\lambda_P = \lambda_K + k,$$

and I was able to give in Table X provisional values of the correction k for the region from λ 3400 to λ 4500.

The corrections k have their origin for the most part in the empirical correction applied by Rowland to his standards in the arc spectrum. It is therefore to be recommended that in a new working over of Kayser's standards the K. system should be so altered by the employment of the corrections k so that it is perfectly identical with the P. T. system. I shall revert to this point at the close of this article.

Although the solution of the problem thus far discussed, the creation of a ***uniform*** system of standards, is readily attainable with our present apparatus, and indeed may be regarded as almost completed, except for the necessary extension of Kayser's standards, the second problem, that of establishing a system of correct relative wave-lengths, encounters difficulties very great and, as it would appear from Kayser's paper, at present insurmountable. This indicates the necessity of beginning now to arrange all spectroscopic measures so that it shall be possible to easily transfer them to another rigorous system of relative wave-lengths, if it later becomes possible to establish such a system. For this purpose, according to what has been said above, nothing more is necessary than that all present measurements should be connected as closely as possible to one of the two above-mentioned uniform systems of standards by the use of a large number of comparison lines. Then, as soon as the system of the P. T. can be made

a rigorous relative system by the application of the corrections C, at present unknown, the same corrections will have to be given to all wave-lengths referred to the P. T.

I can therefore by no means agree with the rather unhappy opinion of Kayser that all previous determinations of wave-lengths, with the sole exception of Hasselberg's, are worthless if a precision of 0.01 tenth-meter is requisite, and that all determinations of wave-length are now useless pieces of work. I am, on the contrary, of the opinion that all measurements which are carried out with sufficient precision, that is, to 0.001, and are rigorously referred to one of the uniform systems above named, will retain their value permanently. The value of these determinations of wave-length is not at all affected by the question of when and with what accuracy the corrections k and C can ultimately be determined. The employment of these two uniform, but not rigorous relative systems of standards can, of course, be only a temporary expedient, and efforts will be made to replace them as soon as possible by a system of the most accurate possible *relative* wave-lengths. The situation is, however, quite different as to the question whether we should not on this occasion also leave the *absolute* system of Rowland and go over to that of Michelson. It is, indeed doubtless to be regarded on first consideration as the ideal arrangement to establish a system whose wave-lengths correspond with absolute precision to the standard meter. But this idea must be regarded as Utopian, as soon as it appears that it will never be fully attainable; indeed, that its attainment would be, not only rather useless, but also even associated with great disadvantages. This case, for instance, is before us in regard to the standards of wave-length. With all respect for the fundamental work of Michelson and Benoit, it would nevertheless be assumed by no one that it furnishes a value of the relation of wave-length to the meter valid for all time. Michelson himself gives a series of corrections which should be taken into account in a repetition of his measurements. Every such repetition of the fundamental measurements which, with the increasing precision of our methods of observation and apparatus, must lead to increasingly sharp values, would involve a change of all wave-length data, if it was desired constantly to work with correct absolute wave-lengths. This would be wholly incompatible with the unity which is absolutely

necessary for such work. It would therefore finally be compulsory, after a longer or shorter time, to adopt my suggestion, namely that we should stop at some one system of *relative* wave-lengths, so that for all later absolute measurements it would be practically necessary to redetermine only the reduction factor, which I have designated by F_o.

The conditions before us are precisely analogous to those in the case of the introduction of the metric system. Inasmuch as the meter is defined as the ten-millionth of the Earth's quadrant, it will be necessary to correspondingly correct the length of the meter after every geodetic measurement of an arc, if we wished to employ an absolutely correct metric measure. But since this is not consistent with the necessary constancy of all units of measure, the only correct escape has been selected, by the legal establishment of a new standard for all time, the idea of the *absolute* correctness of the measure being abandoned. An entirely similar step was taken in astronomy in the definition of the "astronomical unit," that is, in the transition from terrestrial to cosmical dimensions; and it is just this that I have proposed for the transition for the microcosmic system of measurements. This step will in any case be always unavoidable, if the relative measurements are decidedly more accurate, through any range of measurement, than the absolute observations which are made for establishing a connection with the next larger or smaller unit of measure.

If, therefore, according to what has been said, we shall be sooner or later compelled to stop at some system of wave-length which is relatively, but not absolutely, correct, it would be a wholly useless burdening of all spectroscopic work to change at this time from the system of Rowland to that of Michelson. My proposal may therefore be briefly stated as follows:

Let the wave-length of the red line of the cadmium spark in a vacuum measured in air at $+20°$ C. and 760 mm pressure be adopted as

$$\lambda = 6438.6911,$$

as the invariable fundamental value for all time. The measurements of Michelson, Hamy, Fabry, and Perot then will yield values for the lines of *Cd*, *Hg*, *Zn*, *Fe*, *Cu*, *Ag*, *Li*, and *Na*, which were given in

Table XI of my previous paper. These wave-lengths, obtained by the interference method, are without doubt very reliable, but, as I would here expressly point out, they do not at present constitute a *uniform* system, inasmuch as the mode of their origin does not preclude one or the other of these values from being erroneous by a considerable random amount. Therefore, on the one hand, the fundamental references of these lines to the red cadmium line should be repeated, while, on the other hand, their wave-lengths should be adjusted by measurements on numerous good grating plates, and these connections and adjustments should be extended over the whole of the rest of the spectrum. Every strictly relative system of standard lines derived in this way will differ only immaterially from that of Rowland, so that the corrections C to be applied to the earlier measures will always be very small, in many cases indeed negligible.

It is to be hoped that, in accordance with the above-mentioned *most urgent* need, the possessors of large grating spectrographs will first, in continuation of Kayser's iron standards, establish a system of iron lines as uniform as possible, and hence well adjusted, referred to the following standards from the arc spectrum of iron:

4309.533	4383.709	4466.723	5233.132
4315.246	4391.121	4469.552	5302.501
4325.931	4404.913	4476.193	5434.710
4337.207	4415.285	4484.406	5506.970
4336.726	4427.474	4489.915	5586.965
4352.897	4430.785	4494.741	5615.848
4358.675	4442.507	4736.946	5763.219
4367.744	4447.892	4859.928	6065.695
4369.939	4454.558	5002.057	6230.945
4376.089	4461.824	5083.518	6495.213

The first lines of this list, down to that at $\lambda 4495$, were obtained from Kayser's standards by applying the above-mentioned correction k according to Jewell's observations, and they therefore constitute a system as uniform as possible in rigorous agreement with the P. T. The corresponding figures for the preceding part of the spectrum from $\lambda 3400$ to $\lambda 4300$ are similarly obtained by applying to Kayser's wave-lengths in values of k given in Table X of my previous paper. The values from $\lambda 4737$ onward rest on the measures of Perot and

Fabry, and, if their measures are freed from error, constitute a rigorous relative system referred to the cadmium line λ6438.6911. It is also rigorously comparable with the P. T., if the latter are freed from their errors by the application of the corrections C given in my Table VII.

ASTROPHYSICAL OBSERVATORY, POTSDAM,
April 1904.

THE AFTERGLOW OF METALLIC VAPORS IN NITROGEN —A NEW BAND SPECTRUM.

By PERCIVAL LEWIS.

CERTAIN varieties of afterglow or phosphorescence in vacuum tubes have been studied by various investigators.[1] In most of these cases the phenomenon was observed either in mixtures of gases or in single gases which had not been purified with great care; the discharge employed was either the simple induction current, without spark-gap and capacity, or the electrodeless discharge, and in most cases the spectrum, when observed, was found to be continuous. In cases where it was described as discontinuous, details of its character are lacking, and no observations on the ultra-violet region seem to have been made.

In 1899 the writer observed an afterglow in nitrogen which seemed different in several particulars from those previously described.[2] It was found only in nitrogen as pure as could be readily obtained. It was produced only by a strong discharge with spark-gap and condenser in circuit, no trace of it being seen when the simple induction current was used. It appeared with almost equal facility at any pressure from a few millimeters to about 10 cm, and has since been obtained with electrodes close together at a pressure of 35 cm. The spectrum was discontinuous, consisting in the visible region of several strong diffuse lines or bands in the red, yellow, and green. The thin bright path of the discharge was surrounded by a chamois-yellow phosphorescent aureole, filling the entire tube at low pressures and extending 10 or 15 cm in both directions into the connecting tubes. It remained visible several seconds after the passage of the discharge. After each discharge the luminous fog was propagated slowly through the gas in both directions from the electrodes. As in a similar case observed by Warburg,[3] whenever fresh gas was admitted the phos-

[1] KAYSER, *Handbuch der Spectroscopie*, **1**, 249; NEWALL, *Proc. Camb. Soc.*, **9**, 295, 1897; GOLDSTEIN, *Verhand. d. deutschen Phys. Ges.*, 110, 1900.

[2] LEWIS, ASTROPHYSICAL JOURNAL, **12**, 8, 1900; *Ann. der Phys.*, **2**, 249, 1900.

[3] *Arch. des sc. phys. et nat.*, **12**, 504, 1884.

phorescent aureole was driven like a luminous cloud before it. The aureole was instantly destroyed by the admission of very small traces of any foreign gas, such as hydrogen, carbon dioxide, water vapor, and especially oxygen. The nitrogen used was prepared by heating a solution of sodium nitrite and ammonium sulphate, and was carefully freed from oxygen by passing through pyrogallic acid, and from water vapor by passing it through a train of drying tubes. This peculiar form of afterglow could not be obtained from nitrogen prepared from air.

Recently the writer has made further spectroscopic observations of this afterglow, which were extended into the ultra-violet by the use of a quartz spectrograph. The gas was prepared as before, and passed through an alkaline solution of pyrogallic acid, a concentrated solution of KOH, and tubes containing solid KOH, soda-lime, and phosphorus pentoxide. It seems likely that traces of NO are present in nitrogen prepared in this manner.[1]

SPECTRUM OF THE AUREOLE.

Photographs were first taken of the spectrum of the aureole while the discharge was passing. As the aureole extended several centimeters from the electrodes, the use of a bent tube enabled the radiation from a long column of the aureole to fall on the slit of the spectrograph, while it was completely screened from the light of the direct discharge. On account of comparatively feeble luminosity, exposures lasted an hour or more.

The spectra thus photographed were discontinuous, containing many lines and bands in the ultra-violet, besides the lines previously observed in the visible spectrum. The remarkable fact was brought out that a number of metallic lines were present. All the stronger mercury lines, due to vapor diffusing from the pump, were found, the strongest line in the entire spectrum being the mercury line at λ 2537. When aluminum electrodes were used, the stronger aluminum lines were usually, but not always, found. The conditions determining their appearance have not been determined, but it seems likely that they do not appear when very small traces of oxygen or water vapor are present. The stronger nitrogen bands of Deslandres' second and third groups appear on the plates, but none of the first group

[1] KREUSLER, *Ann. der Phys.*, **6**, 419, 1901.

could be detected visually with a pocket spectroscope of great power. There were also a number of other lines and bands which have not yet been identified.

With platinum electrodes the aureole was unchanged in general appearance, and the visible spectrum was the same as before, but the ultra-violet region was somewhat different. The mercury lines were strong. The nitrogen bands were relatively weak. The platinum lines at λλ 2629, 2655, 2660, 2706, 2734, 2830, 2930, 2998, 3060, 3157, and 3408 were apparently present. Their coincidence with lines of the spark between platinum terminals in air seemed perfect. Several new bands with edges toward the violet were especially prominent.

With iron electrodes the same band spectrum was given as with platinum, but no metallic lines, except those due to mercury, could be found—except possibly the strong iron lines at λ 2788 and λ 4325.

With zinc electrodes the strongest zinc lines were found—λλ 4811, 4722, 4680, 3345, 3303, 3282, 3072, 3035, 3018, 2801, 2771, 2756.

Some of the results obtained are shown in Plate IV. *A* is the spectrum of the direct discharge, without condenser, showing the bands of the positive column and the negative bands; *B* is the spectrum with condenser in circuit, showing both lines and bands. The exposure lasted about five minutes. *C* is the spectrum of the aureole at a pressure of 5 mm, exposure nearly one hour. The aluminum and the mercury lines are strong, while the nitrogen bands of the second group are weak. *D* is the same, taken at a different time with another tube at a pressure of 15 mm. The nitrogen bands are stronger, and several new lines and bands appear. The differences in relative intensity of *C* and *D* have not been accounted for. It may be due to changed electrical conditions. Two very strong and characteristic lines in the yellow and green are seen to have no correspondence with the nitrogen line or band spectrum, as shown in *B* and *A*.

Eye observations were made of these lines, and of another in the red which does not appear on the plates, with a spectroscope of great dispersion, and more exact determinations of wave-length were made than those previously published. There was a strong diffuse line at λ 6245, with a faint line on each side, of wave-lengths λ 6320 and λ 6175. The yellow line was separated into three pairs, of wave-lengths 5865–

45, 5800–5780, and 5760–35. The central pair was the strongest. These lines lie very near the mercury pair λ 5790–69, but comparison with the usually much stronger, but in this case barely perceptible, green mercury line makes it doubtful whether that pair would be visible under these conditions. The green line was found to be a band of fine lines, with head toward the red at λ 5408. A fainter band with sharp edge toward the violet was seen at λ 5005. All these lines and bands lie near lines or bands of the known nitrogen spectrum, but their general appearance and relative intensities indicate a different origin. The red, yellow, and green lines are the most characteristic and uniform of the new lines found in the aureole.

J is the spectrum of the aureole with platinum electrodes; *K*, the same with iron electrodes. *L* is the spectrum of the aureole in a commercial tube filled with NO_2, which also shows an afterglow (produced by either the simple induction or the condenser discharge). The visible spectrum, even when examined with high dispersion, appeared perfectly continuous, with a strong maximum in the red and yellow; in the ultra-violet the second group of nitrogen bands appears. The glass walls prevented observation any further into the ultra-violet. The spectrum of the direct discharge appeared to be identical with that of nitrogen.

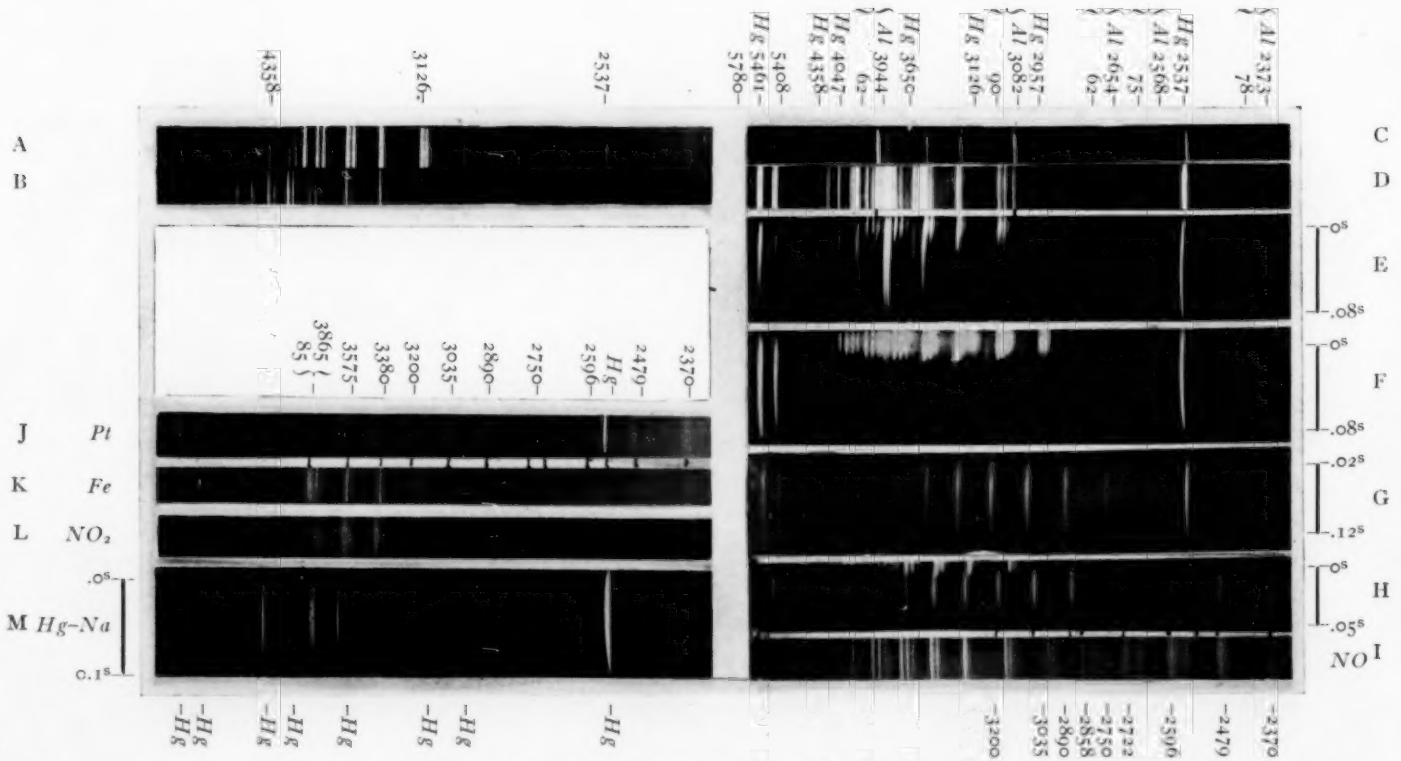
Photographs taken in connection with a previous investigation,[1] using a large glass prism and photographing simultaneously the spectrum of each part of the tube, showed that the aluminum lines at λ 3944 and λ 3962 were uniformly strong throughout the aureole, up to a distance of about 5 cm from the electrodes; but when there was no aureole showing the afterglow these lines could be found only in the immediate vicinity of the electrodes. Without the condenser and spark-gap, the aluminum lines appeared only at the negative electrode. This indicates that aluminum vapor or electrons must have diffused throughout the phosphorescing mass of gas, taking part in the processes giving rise to the afterglow.

SPECTRUM OF THE AFTERGLOW.

Observations were next made on the spectrum of the afterglow which persisted when the current was interrupted. In front of the spectrograph slit was placed a large metallic disk, provided with

[1] LEWIS, ASTROPHYSICAL JOURNAL, **17**, 263, 1903.

PLATE IV.



SPECTRA OF THE AUREOLE AND AFTERGLOW IN NITROGEN.

ten projecting sectors (like Fizeau's toothed wheel), with intervals of several centimeters between them, which acted as intermittent screens when the disk was slowly rotated at a uniform rate by an electric motor. An adjustable contact allowed the current to be broken at any given phase of the sectors. These traveled downward in front of the slit, exposing the top first and the bottom about 0.1 second later. There was an integral exposure of each part of the slit about 0.3 second in duration, and then the next sector crossed the field, the circuit was closed, and the process was repeated. Some very interesting results were obtained, as shown in Plate IV, which is described below:

E. Pressure 15 mm; exposure, 2 hours. The current was broken when about one-fifth of the slit was exposed, so that the top shows the spectrum of the discharge. When the sector was at 0^s the current was interrupted, and beyond this point the spectrum of the afterglow proper appears, the lower part of the slit not being exposed until about 0.08 sec. after the discharge. The mercury line and a number of the characteristic afterglow lines and bands appear with almost undiminished intensity throughout this interval. The aluminum pairs at 3962–44 and 3093–82 appear in the spectrum of the afterglow, and persist fully 0.06 sec. In another plate they were found up to the very limit of 0.08 sec. Before each exposure the spectrograph was removed, the sectors were adjusted, and eye observations were made to be certain that no light from the direct discharge could enter below the assigned points on the slit. Even when the sectors were set so that no light at all from the discharge could pass beyond the wheel, a pocket spectroscope showed the characteristic visible afterglow lines and alongside of the band at λ 5408 was always seen the green mercury line at λ 5461, as shown in *D* and *E*.

F. Pressure about 4 mm; otherwise the same as above, but with slightly wider slit. The mercury line at λ 2537 seems as strong as before, but the other mercury lines are very weak. The aluminum lines do not appear. Some of the characteristic bands are stronger than at the higher pressure, some weaker. The stronger nitrogen bands of Deslandres' third group appear, and are persistent in the afterglow, although the bands of the second group disappear in this and the preceding case almost simultaneously with the break in the current.

G. Pressure 20 mm; exposure, 2 hours. The sectors were set so that exposure did not occur until about 0.02 sec. after the discharge ceased, the lower end of the slit being exposed 0.1 sec. later. This appears to be a continuation of the effect shown in *F* rather than of *E*, as might be expected from the pressure. Many such irregularities of relative intensity, and other characteristics which have not yet been accounted for, occurred frequently.

H. This is similar to *F*, except that the exposure is longer. This is of special interest because of the absence of the mercury line at λ 2537. This was due to the formation of a moist plug in a phosphorus pentoxide drying tube, which prevented the diffusion of mercury vapor from the pump. This is additional evidence that the mercury vapor plays no essential part in the origin of the strange bands and lines.

I. The spectrum of the discharge through *NO*, giving the bands of the second and third group for comparison with the above. This tube gave no afterglow with any kind of discharge, although a commercial *NO* tube gave the same afterglow as the commercial NO_2 tube (*L*).

The following metallic lines were found in the spectrum of the aureole, the strongest occurring also in the afterglow: mercury—λλ 5790, 5769, 5461, 4358, 4047, 3650, 3126, 2957, and 2537; aluminum—λλ 3962, 3944, 3093, 3082, 2662, 2654, 2575, 2568, 2378, 2373.

The lines of zinc and platinum found in the spectrum of the aureole have already been given. Those of zinc do not appear in the spectrum of the afterglow, and as yet the spectrum of the afterglow with platinum electrodes has not been photographed.

E. Wiedemann and Schmidt[1] have shown that metallic vapors will fluoresce under the action of light, but this seems to be the first case on record where phosphorescent radiation has been obtained from metallic vapors a measurable time after the electrical or luminous stimulus has ceased. Crew[2] found that the radiation of metallic vapors in the electric arc ceased within 0.001 sec. after the stopping of the arc. Whether the effect is of chemical origin, due to combinations between the metals and the gas, or whether it is caused by a readjustment of electrical equilibrium following the disturbance

[1] ASTROPHYSICAL JOURNAL, **3**, 207, 1896. [2] *Proc. Am. Acad.*, **33**, 337, 1898.

caused by the current, remains to be discovered. Small traces of oxygen are fatal to the afterglow, perhaps on account of the avidity with which this gas combines with any metallic vapors or free gaseous ions which may be present.

A NEW BAND SPECTRUM.

On most of the plates showing the afterglow spectrum there are a number of bands, some with edges toward the violet, which have not yet been identified. They are different in position and appearance from any of the known nitrogen bands. They appear when no mercury vapor is present, and cannot be due to it. They are likewise found when no phosphorus pentoxide drying tubes were used, so that they are not due to vapors from that. Although every effort was made to avoid contamination, it is practically impossible to avoid impurities from the materials used to generate the gas, from the drying tubes, and from the grease on the stop-cocks (a mixture of vaseline and paraffin), and the lines may arise in this way.

Comparison with *I* (spectrum of *NO*) may give a clue to the origin of some of these bands. In *F*, *G*, and *H*, several bands of the afterglow are evidently coincident with the strong bands of the third group in *I*. Still stronger bands of the afterglow in *F*, *G*, *H*, and *J*, of approximate wave-lengths 2750, 2890, 3035, and 3200, with edges toward the violet, apparently coincide with very weak bands in *I*. Other bands of wave-lengths 3380, 3575, and 3805 cannot be found on *I*, perhaps because strong bands of the second group overlie them. Some of these bands are almost coincident with bands of the second group, but appear to be different. Other bands at λ 4130 and λ 4540 are found on *D*, and *E* but not on the other plates. It seems possible that some of these bands are due to the presence of *NO*. None of them are to be seen in the spectrum of NO_2 (*L*), but they may be due to other oxides of nitrogen, or to compounds of nitrogen with impurities.

Especially prominent on *D*, *E*, and *M*, in the afterglow, are two bands of wave-lengths near 3865 and 3885. They much resemble the cyanogen bands found in the spectrum of the Bunsen flame,[1] and may possibly be due to that compound. The wave-lengths of these bands are given by Eder as 3873 and 3890.

[1] EDER, *Wiener Denkschriften*, **57**, 551, 1890.

No special attempt was made at accuracy in these determinations of wave-length, which may be in error by 5 units or more. Further measurements will be postponed until a spectrograph of much greater dispersion is available.

It was found in previous investigations that the afterglow was destroyed by heating metallic sodium in the tube, presumably thus reducing any oxides. It seems, however, that this effect was probably due to impurities in the sodium; for after heating it repeatedly to drive off volatile ingredients, it was found that the afterglow could be obtained although not quite so strong as before. It is a remarkable fact, however, that the sodium lines did not appear in the spectrum of the aureole or of the afterglow.

Mercury-sodium amalgam was heated in the tube. In this case the afterglow appeared in full strength, but when the tube was heated the mercury vapor seemed to be dominant, giving a greenish-white color to the afterglow. It was a real afterglow of the mercury vapor, persisting more than 0.1 sec., as shown by *M*. The mercury lines are very much enhanced, the bands of the afterglow somewhat weakened. It must be repeated that numerous efforts failed to find any afterglow of any metallic vapor unless the conditions were such as to cause an afterglow in the gas.

When the mercury-sodium amalgam was used, the sodium lines were also absent, even when the tube was highly heated. This is singular, considering that sodium is so much more volatile than aluminum.

Goldstein[1] has described some forms of afterglow which may be similar to that here described. He also refers to some unpublished observations of Hertz on the spectrum of an afterglow in nitrogen giving a discontinuous spectrum, but precise details of such spectra have never been published, and no one has apparently observed the very significant fact that metallic vapors may take part in the phenomenon.

The writer will further investigate the electrical conditions accompanying the afterglow, and endeavor to find the origin of the new bands described. It will also be of interest to find whether such

[1] *Verhand. d. deutsch. phys. Ges.*, p. 110, 1900.

afterglows of metallic vapors may take place in other atmospheres than nitrogen.

The results so far obtained may be summarized as follows:

1. The afterglow described occurs only in nitrogen chemically prepared, dried and purified as far as possible, but probably containing traces of *NO*.

2. This afterglow can be produced only by a strong condenser discharge.

3. Its spectrum is discontinuous, consisting of lines and bands.

4. Some bands are those of Deslandres' third group, others are of unknown origin. Some of the lines are due to mercury and aluminum vapor, others are unidentified.

5. The mercury and aluminum vapors phosphoresce at least 0.1 sec. after the discharge has ceased. There is no afterglow of the metallic vapors unless there is an afterglow in the gas.

6. The mercury lines appear equally at all pressures; the aluminum lines are usually stronger at relatively high pressures—2 cm or more—but are sometimes altogether absent.

7. With heated mercury-sodium amalgam in the tube, the afterglow still persisted, but seemed largely localized in the mercury vapor, the intensity of the other afterglow lines, being less, and the afterglow turning white.

Mr. P. E. Rowell rendered very valuable assistance during this investigation.

UNIVERSITY OF CALIFORNIA,
Berkeley, May 24, 1904.

NOTES ON THE SPECTRA OF NITROGEN AND ITS OXIDES.

By PERCIVAL LEWIS.

THE SPECTRUM OF THE ELECTRODELESS DISCHARGE.

SOME general observations on the spectrum of the electrodeless discharge in various gases and in gaseous mixtures showing an afterglow have been made by J. J. Thomson[1] and by Newall,[2] but no systematic work has been done on such spectra. If it be assumed that the ring discharge is due solely to electromagnetic induction, there can be no passage of electricity between electrodes and the gas, and no finite discontinuities, nor even a potential gradient in the path of the current. This, and the fact that the pressure in the gas is much less than that ordinarily employed in vacuum tubes, render it possible that some characteristic peculiarities may be observed in such spectra.

Nitrogen is a particularly interesting subject to study in this connection, on account of the multiplicity of its spectra, and because it has several groups of bands which behave quite differently under different physical conditions. As it is often assumed that the spectrum of the negative pole is due to cathode rays, it is also of interest to see whether these bands appear when there is no cathode.

Photographs were taken with a quartz spectrograph. The discharge tube was a spherical glass bulb, about four inches in diameter and provided with a quartz window. Around it was a pasteboard cylinder on which were wound eight turns of heavy wire in circuit with spark-gap and condenser. The pasteboard cylinder was moistened to screen the tube from electrostatic effects, and the helix connected to earth. The ring was always clearly defined.

The nitrogen used was prepared by heating a solution of sodium nitrite and ammonium sulphate.[3] It was freed from traces of oxygen by bubbling through an alkaline solution of pyrogallic acid, and from

[1] *Phil. Mag.*, **32**, 1892. [2] *Proc. Camb. Soc.*, **9**, 295, 1897.

[3] Nitrogen prepared in this manner contains traces of *NO*. (See KREUSSLER, *Ann. der Phys.*, **6**, 419, 1901.)

carbon dioxide and water vapor by passing it through a concentrated solution of caustic potash and several drying tubes.

The discharge appeared at pressures of about 0.1 mm. At first it was of coppery-pink color, characteristic of pure nitrogen. At somewhat lower pressures it turned white, as likewise observed by J. J. Thomson; but the spectroscope showed that this whiteness was due to mercury vapor from the pump, not to any change in the nitrogen, nor even to carbon compounds, the spectrum of which was vanishingly weak. Photographs were usually taken at pressures of about 0.05 mm.

Deslandres[1] showed that in the spectrum of the positive column in nitrogen there are three distinct groups of bands, which are differently affected by physical conditions. The first group comprises most of the visible spectrum, and is attributed by him to pure nitrogen. The second, which begins in the violet and extends to about λ 2800, is supposed by him to be due to nitrogen and hydrogen. The third begins about λ 3000 and extends to about λ 2100. This group is considered by Deslandres to be due to traces of oxygen, forming an oxide, as metallic sodium heated in the tube will eliminate it.

In the photographed spectrum of the ring discharge, shown in *A*, Plate V, the third group is entirely lacking. The second group is strong. The first group, as observed visually, was fairly strong. The difference between this spectrum and that of the simple induction discharge with electrodes is shown by comparison with *B* (spectrum of the negative pole) and *C* (spectrum of the positive column), taken at a pressure of about 5 mm. The differences may depend on differences of pressure as well as of electrical discharge. The stronger mercury lines are all present, as well as several other sharp lines which have not been identified.

It is interesting to note that the negative bands appear on the plates, as may be seen by comparison with *B*. The only apparent way to reconcile this with the view that the negative bands are due to cathode rays seems to be to assume that there are electrostatic discharges from the glass walls, which thus act as electrodes. This is in accordance with some other results obtained by the writer.[2] Hardèn[3] and

[1] *Ann. Chim. et Phys.*, **15**, 46, 1888.

[2] ASTROPHYSICAL JOURNAL, **17**, 266, 1903.

[3] *Physikalische Zeitschrift*, **5**, 74, 1904.

Lecher[1] have recently shown that the ring discharge as ordinarily produced is largely due to electrostatic discharges from the walls of the tube.

Some believe that the spectrum of the negative pole is due to the higher temperature at the cathode. It was found that the negative glow could easily be produced at any point of the bulb by simply placing the finger at that point , and no very high temperature seems to be possible under such conditions; nor is the ring discharge sufficiently violent to allow us to explain the appearance of the negative bands as a result of the intensity, as suggested by Goldstein.[2] It seems highly probable that the negative bands are not primarily due either to high temperature or to great intensity of discharge, although some recent work of Hemsalech[3] shows that the negative glow may be greatly intensified by heating the electrode.

THE THIRD GROUP OF BANDS.

The third group of bands, with principal maxima at λλ 2370, 2479, 2596, 2722, and 2858, appear in the spectrum of the negative pole (*B*) and of the positive column (*C*), at pressures of 5 mm. They also appear in the spectrum of the simple spark between platinum terminals in nitrogen at 1 atmosphere (*H*) and at 2 atmospheres pressure (*I*). They do not appear when a small capacity is placed in parallel with the tube (*D*). If these bands be due to an oxide of nitrogen, as Deslandres suggests, it might be expected that the more intense discharge with the capacity would permanently dissociate this oxide and cause its spectrum to disappear.

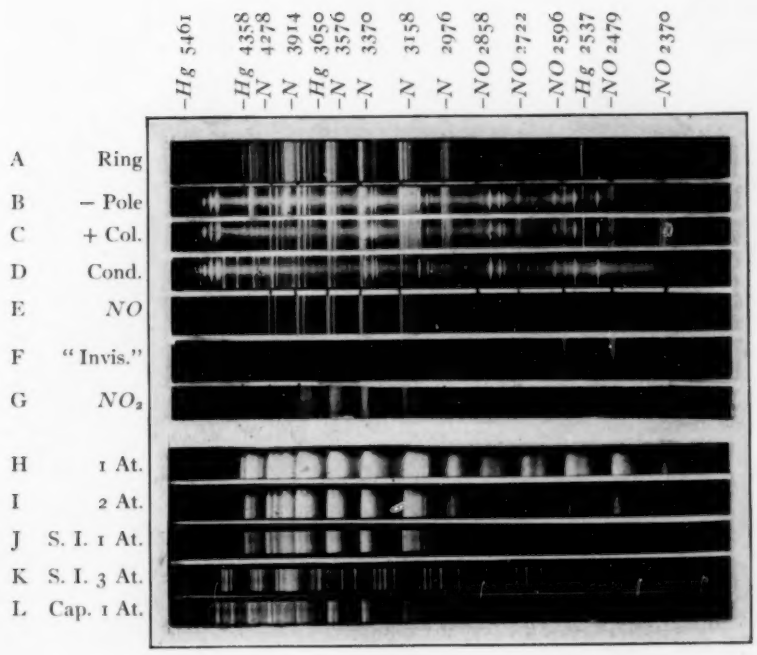
(A zinc comparison spectrum is superimposed on *B*, *C*, and *D*.)

E gives the spectrum of a tube filled with pure dry *NO*. It is much like *C*, but the bands of the third group are relatively much stronger. The same spectrum was obtained with external electrodes. giving a very feeble current, with inner exectrodes and a simple induction current, and with a small capacity in circuit. It was observed, however, that the tube fluoresced brilliantly during the first moments of the discharge; but this fluorescence gradually died out, and more rapidly as the intensity of the discharge was increased—

[1] *Ibid*, **5**, 179, 1904. [2] *Wied. Ann.*, **15**, 280, 1882.

[3] *Mem. Manchester Phil. Soc.*, **48**, No. 10, 1904.

PLATE V.


-Hg 5461
-Hg 4358
-N 4278
-N 3914
-Hg 3650
-N 3576
-N 3370
-N 3158
-N 2976
-NO 2858
-NO 2722
-NO 2596
-Hg 2537
-NO 2479
-NO 2370
A Ring
B − Pole
C + Col.
D Cond.
E NO
F "Invis."
G NO_2
H 1 At.
I 2 At.
J S. I. 1 At.
K S. I. 3 At.
L Cap. 1 At.

presumably the effect of the gradual dissociation of the oxide, resulting in a diminished ultra-violet radiation. This is in accordance with results previously obtained by the writer,[1] which showed that a brilliant fluorescence of the tube accompanied the passage of a constant stream of fresh gas through the tube, and that this fluorescence was due to the ultra-violet radiation of the third group. These bands are attributed to *NO* on *B*, *C*, *E*, and *F*, as the evidence seems to justify Deslandres' view. As oxides likewise exist in nitrogen prepared from air, it seems probable that no one has ever studied the spectrum of pure nitrogen.

THE INVISIBLE DISCHARGE.

In the previous investigation, alluded to above, it was found that often when the pressure was too high for the discharge to pass, the glass fluoresced brightly around the electrodes. It has since been found that very brilliant effects may be obtained in this way at pressures of from 10 to 20 cm, when there was absolutely no visible discharge except an occasional feeble brush from the electrodes. Sometimes the tube fluoresced over the entire distance between the electrodes. The spectrum of this invisible discharge is shown in *F*. The pressure was about 15 mm, and the exposure was 30 minutes. The third group of bands is very strong, and the edges of the second group very distinct. It may be imagined that with such relatively feeble electrical disturbances the nitric oxide might be excited to radiation without dissociation. These results may have some connection with those recently obtained by Sanford.[2] He found that the metal of condenser plates or between condenser plates emitted invisible radiations with a maximum lying near λλ 3500–3700. This is the region of the strongest bands of the second group.

SPECTRUM OF NO_2.

The spectrum of the discharge in this compound seemed to be identical with that of nitrogen, so far as its radiation was transmitted through a commercial glass tube. The spectrum of the phosphorescent aureole showing the afterglow is given in *G*. This spectrum, even when examined with high dispersion, seemed perfectly continu-

[1] ASTROPHYSICAL JOURNAL, **12**, 8, 1900; *Ann. der Phys.*, **2**, 49, 1900.

[2] *Physical Review*, **18**, 366, 1904.

ous in the visible region, with a strong maximum in the red and yellow. In the ultra-violet it seems to consist solely of the bands of the second group.

THE SPECTRA OF NITROGEN AT HIGH PRESSURES.

H, *I*, *J*, *K*, and *L* are reproductions of some photographs taken by Mr. Joel Stebbins in this laboratory while he was a fellow of Lick Observatory.

H is the spectrum of the simple spark between platinum electrodes in nitrogen at 1 atmosphere pressure; *I* the same at a pressure of 2 atmospheres; exposure, 30 minutes. Little or no difference can be observed, except that the entire spectrum is weaker at the higher pressure, and that a few of the stronger platinum lines appear.

J is the spectrum of the spark with a capacity and a self-induction of several hundred turns in the circuit, as a pressure of 1 atmosphere; exposure, 6 minutes. It is much like *I*, except that a number of platinum lines appear.

K is the same as above at a pressure of 3 atmospheres. The character of the spectrum is entirely altered. It consists mostly of lines, with a few of the stronger bands of the second group.

L is the spectrum at 1 atmosphere, with capacity, but no self-induction; exposure, 5 minutes. This likewise consists of lines with a few bands, but some lines appear which are not seen on *K*, and conversely, the new lines being apparently air lines.

The lines which are enhanced on *K* appear to be the strong platinum lines λλ 2630, 2655, 2660, 2706, 2734, 2794, 2830, 2988, 3043, 3064, 3157, 3204, 3283, 3468, 3485, 3628, and others.

Comparison of *J*, *K*, and *L* shows that increased pressure with constant self-induction has the same effect as increased self-induction at constant pressure—that is, the metallic lines are enhanced and the air spectrum weakened. A further investigation of this effect is in progress in this laboratory.

UNIVERSITY OF CALIFORNIA,
Berkeley, May 22, 1904.

COMET 1903 BORRELLY AND LIGHT-PRESSURE.

By S. A. MITCHELL.

IN view of the great interest at present shown in the pressure of light and its theories, it would probably be of some consequence to see what bearing the comet of 1903, Borrelly, has on the question. That a ray of light exerts a pressure on any surface on which it impinges comes as a direct result of the Electromagnetic Theory of Light published in 1873 by Clerk Maxwell,[1] and, as was shown by Bartoli[2] in 1876, can be deduced from the second law of thermodynamics. The pressure of sunlight at the surface of the Earth Maxwell computed from the known constants of solar radiation to be 0.592×10^{-10} grams per sq. cm. (This value is changed somewhat by using the latest determination of the solar constant as derived by Langley and Abbot at the Smithsonian Institution.) At the Sun's surface, as was pointed out by Arrhenius,[3] the radiation is much more powerful, and amounts to 2.75×10^{-3} grams per sq. cm. If, then, we imagine at the Sun's surface a cubical block of water of 1 cm edge, the pressure of light on it would be one ten-thousandth part of its weight, since superficial gravity at the Sun is 27.47 times what it is on the Earth's surface. If we decrease the size of the water particle, the pressure diminishes as the square of the edge and the weight as the cube. Consequently, for a little cube of water with an edge 10^{-4} cm, *i. e.*, one micron, or μ, the pressure of the Sun's light on it is exactly equal to its weight, and for a smaller cube of water the pressure would be greater than the weight, and the particle would act as if gravity had become negative. If the drop of water were spherical in shape instead of cubical, the critical diameter, at which the repulsion due to light-pressure would be equal to the attraction of gravity would be $1.5\,\mu$. For other substances the diameter is inversely proportional to the specific gravity.

The vapors of comets, as we are informed by the spectroscope,

[1] *Electricity and Magnetism*, § 792. [2] *Il Nuovo Cimento*, **15**, 195, 1883.

[3] *Physikalische Zeitschrift*, **2**, 81, 1900; also, *Lehrbuch der kosmischen Physik*, 150, 1903.

appear to consist largely of hydrocarbons with a specific gravity about 0.8, and for these the critical diameter would be about 1.9 μ. If the vaporized portions of the comet form drops whose diameters are greater than this value, a tail will be formed pointing toward the Sun; but if the diameters are less than 1.9 μ, the tail will point away from the Sun. If it should happen that drops of different sizes are formed—and there seems to be no reason why this should not be possible—the comet will have several tails, as in the comet of 1744, which had five.

Such, in brief, is the theory of Arrhenius,[1] which explains away readily enough a great many of the difficulties connected with comets and their tails.

Since the light-pressure depends directly on the intensity of the Sun's radiation, which decreases inversely as the square of the distance, as is also the case with gravity, the ratio of pressure to weight is therefore a constant independent of the distance from the Sun. The manner in which this ratio is found was first shown by Bessel[2] in 1836, who computed the magnitude of the repulsive force from the curvature of the tail of the comet of 1811. Bredichin[3] more recently, from measures of many comets' tails, has found them to be of four different types, in which the repulsive forces are respectively 18.5, 3.2, 2.0, and 1.5 times the attraction of gravity; the straight tail, according to his ideas, consisting of hydrogen, the plume-like tail of hydrocarbons, and the short stubby one of metallic vapors, chief among which are iron and sodium. The electrical force, on which Bredichin explains his repulsions, has been shown by Lebedew[4] not to have a sound physical basis.

This objection cannot be raised to the principle of Arrhenius. That light exerts a pressure has been shown by Lebedew,[5] and Nichols and Hull;[6] the latter, indeed, have succeeded[7] in producing

[1] See the interesting articles by Cox, *Popular Science Monthly*, **60**, 26 , 1902.

[2] *A. N.*, **13**, 185, 1836.

[3] *Annales de l'Observatoire de Moscou*, (**2**) **1**, 45, 1886.

[4] Astrophysical Journal, **16**, 155, 1902.

[5] *Annalen der Physik*, (**4**) **6**, 433, 1901; Astrophysical Journal, **15**, 60, 1902.

[6] *Physical Review*, **13**, 307, 1901; Astrophysical Journal, **15**, 62, 1902.

[7] Astrophysical Journal, **17**, 352, 1903.

a laboratory comet's tail, although, as pointed out by them, other forces than light-pressure probably helped to give the repulsion.

However, a rigid application of the theory of Maxwell is possible only when the body acted upon is large compared with the vibrations of light itself. When the body is of a size approximating the wave-length of light, Schwarzschild[1] has shown that the maximum value of the repulsive force is about twenty times the attraction of gravity.

The measures by Sebastian Albrecht[2] of photographs of comet 1903, Borrelly, give the data for determining the curvature of the comet's tail and the magnitude of the repulsive forces forming the tail.

Using the parabolic elements by Perrine in *Lick Observatory Bulletin* No. 47, it is possible to compute the velocity of the comet along the radius vector and perpendicular thereto. Added to these two, there is a third velocity caused by light-pressure along the radius vector, but directed away from the Sun, and the resultant of these three gives the motion in the comet's tail. Combining together the motions to and from the Sun along the radius vector, the direction of the tail comes as the resultant of two velocities. As is easily seen, the tail must lag behind the radius vector. Conversely, knowing the velocity of the comet perpendicular to the radius vector, and the angle the tail makes with this line to the Sun, it is possible to calculate the velocities caused by light-pressure.

As it is impossible for the comet's tail to be ahead of the radius vector, or, using Albrecht's notation, for the angle between the radius vector and the tail to have a negative sign, such angles were neglected in the calculation for finding the values of the repulsive forces which follow:

[1] *Sitzungsberichte der math.-phys. Classe der k. b. Akademie der Wissenschaften zu München*, **31**, 293, 1901.

[2] *Lick Observatory Bulletin* No. 52; ASTROPHYSICAL JOURNAL, **19**, 121, 1904.

RATIOS BETWEEN REPULSION FORMING TAIL AND GRAVITY.

Plate No.	Date, 1903	P. S. T.	Principal Tail		Secondary Tail	
			Angle between Radius Vector and Tail	Value of Repulsive Force	Angle between Radius Vector and Tail	Value of Repulsive Force
1	June 22	$14^h\ 42^m$	− 1°.5	.....	+15°	1.9300
2	June 23	13 37	+ 1.8	16.804	+15	1.9571
3	June 26	13 30	− 2.3	.....	+12	2.6549
4	June 29	12 46	+ 2.5	12.674	+16	1.8802
5	June 30	13 33	+ 1.2	26.649	+17	1.8845
6	July 1	13 48	+ 4.4	7.318	+18	1.7751
7	July 12	8 52	+ 2.1	17.093	+20	1.6853
8	July 13	9 12	+ 4.5	8.057	+19	1.8210
18	July 20	10 58	+ 0.4	(99.598)	....	.....
19	July 20	14 40	− 0.4	.....	....	.....
20	July 23	11 45	+ 1.5	27.820	+16	2.5240
21	July 24	11 45	+ 3.9	10.864	+18	2.2794
22	July 25	12 00	+ 3.5	12.322	+22	1.8560
23	July 26	11 48	+ 0.6	(73.250)	+25	1.6755
24	July 27	10 20	+12.5	(3.505)	+25	1.6514
25	July 28	11 48	+ 4.2	10.852	+24	1.7083
26	July 29	11 53	+ 5.9	7.871	+26	1.6822
27	Aug. 11	8 49	+ 0.6	(114.940)	+35	1.7255
28	Aug. 12	9 00	+ 1.2	(58.925)	+36	1.6926
29	Aug. 13	9 12	+ 2.0	35.580	+44	1.3980
30	Aug. 14	9 05	+ 2.2	35.042	+43	1.4385
31	Aug. 15	8 55	+ 2.9	29.634	+43	1.6325
32	Aug. 18	8 36	+. 1.4[1]	(77.973)	+54	1.3694
				Mean 18.47		Mean 1.824

In taking the average for Tail 1, the quantities coming from angles less than 1°, or from angles widely discordant, as in Plate 24 were not included. Thus, neglecting the values in ( ), the mean value of the repulsive force is 18.47 times gravity. Naturally the values of the repulsive force for Tail 2 agree much better among themselves, and their mean is 1.824.

The last four values in the table, with a mean of 1.460, seem to show the existence of a third tail, and this is corroborated from the photographs of August 12 and 15.

The angles between the radius vector and the tail were computed from the mean values of the repulsive forces above determined, and results are given in the following table, where *O* represents the values

[1] By mistake this angle was printed +14°.1 in Mr. Albrecht's measures. The value of the repulsive force was not included in the mean.

as measured by Albrecht, and C those computed. Values are also given for a third tail, for the last four measures only.

ANGLES BETWEEN TAILS AND RADII VECTORES.

Plate	Date, 1903	Principal Tail			Second Tail			Third Tail		
		O_1	C_1	O_1-C_1	O_2	C_2	O_2-C_2	O_3	C_3	O_3-C_3
		°	°	°	°	°	°	°	°	°
1	June 22.....	− 1.5	+1.6	− 3.1	+15.2	+16.0	−0.8	.....	.....	.....
2	June 23.....	+ 1.8	+1.6	+ 0.2	+15.1	+16.1	−1.0	.....	.....	.....
3	June 26.....	− 2.3	+1.7	− 4.0	+11.5	+16.5	−5.0	.....	.....	.....
4	June 29.....	+ 2.5	+1.7	+ 0.8	+16.4	+16.8	−0.4	.....	.....	.....
5	June 30.....	+ 1.2	+1.7	− 0.5	+16.5	+17.0	−0.5	.....	.....	.....
6	July 1.....	+ 4.4	+1.8	+ 2.6	+17.6	+17.1	+0.5	.....	.....	.....
7	July 12.....	+ 2.1	+2.0	+ 0.1	+20.4	+19.0	+1.4	.....	.....	.....
8	July 13.....	+ 4.5	+2.0	+ 2.5	+19.2	+19.2	0.0	.....	.....	.....
18	July 20.....	+ 0.4	+2.1	− 1.7	.....	+20.9	.....	.....	.....	.....
19	July 20.....	− 0.4	+2.2	− 2.6	.....	+20.9	.....	.....	.....	.....
20	July 23.....	+ 1.5	+2.2	− 0.7	+16.1	+21.8	−5.7	.....	.....	.....
21	July 24.....	+ 3.9	+2.3	+ 1.6	+18.0	+22.1	−4.1	.....	.....	.....
22	July 25.....	+ 3.5	+2.3	+ 1.2	+22.1	+22.5	−0.4	.....	.....	.....
23	July 26.....	+ 0.6	+2.4	− 1.8	+24.6	+22.6	+2.0	.....	.....	.....
24	July 27.....	+12.5	+2.4	+10.1	+25.2	+23.1	+2.1	.....	.....	.....
25	July 28.....	+ 4.2	+2.5	+ 1.7	+23.9	+23.7	+0.2	.....	.....	.....
26	July 29.....	+ 5.9	+2.5	+ 3.4	+25.8	+24.0	+1.8	.....	.....	.....
27	Aug. 11.....	+ 0.6	+3.7	− 3.1	+34.9	+33.4	+1.5	.....	.....	.....
28	Aug. 12.....	+ 1.2	+3.8	− 2.6	+36.1	+34.1	+2.0	.....	.....	.....
29	Aug. 13.....	+ 2.0	+4.1	− 2.1	+43.6	+36.1	+7.5	+43.6	+42.4	+1.2
30	Aug. 14.....	+ 2.2	+4.2	− 2.0	+43.1	+36.4	+6.7	+43.1	+42.7	+0.4
31	Aug. 15.....	+ 2.9	+4.6	− 1.7	+42.6	+39.5	+3.1	+42.6	+45.8	−3.2
32	Aug. 18.....	+ 1.4	+5.9	− 4.5	+54.3	+46.3	+8.0	+54.3	+52.5	+1.8

(If the last four measures show a third tail, the value of the repulsive force for the secondary tail would be 1.91 times gravity, and the the values O_2—C_2 would be slightly changed.)

The agreement of results is fairly satisfactory. Errors of observation[1] in measuring the position angles of so faint and ill-defined an object as a widespread comet's tail must be quite large.

The light-pressure theory makes it plain why the angles between the radius vector and the tail continually increase up to perihelion. This explanation is simpler than to imagine, with Mr. Albrecht, that "this increase may be due to the fact that the speed of the nucleus in the orbit was rapidly accelerated up to the date of perihelion passage, August 27, thereby causing this tail to lag behind more and more."

[1] Mr. Albrecht has informed me that the "published position angles of the tails are the means of two entirely independent sets of measures. The first set was made before the position angles of the radius vector were computed, and, intentionally, three weeks of time were allowed to eclipse before making the second set, so as to be influenced as little as possible by the first measures in judging the directions of the tails."

However, there seems to be a lagging even behind the direction given by the repulsive force, as is seen from the August measures on the principal tail; or, in other words, the value of the repulsive force may increase as the comet approaches the Sun. This increase, shown by the values derived, may be apparent only, being due to errors of observation in determining the position angle of the tail; but it is more likely that it is, in part, at least, real. There is then some force other than light-pressure at work, and this force undoubtedly comes from the more violent action of the gases liberated as the comet approaches the Sun.

On July 24 an interesting break was observed in the tail, and on this night two photographs were taken at the Yerkes Observatory, one at Nanterre, France, and one at the Lick. From measures of Yerkes and Nanterre photographs, Professor Barnard[1] determined a motion of recession of 7 miles per second relative to the Sun; and Mr. Albrecht, by measures on a slightly different portion of the detached area, from combinations of all four photographs, found the recession to be at the rate of 13 miles per second from the Sun. On this date, as is determined from the comet's orbit, the velocity of the head relative to the Sun was 22 miles per second, and of the tail 407 miles per second; this velocity, it may be noted, is independent of the nature of the force emanating from the Sun, whether light-pressure, electrical, or some other. As the motion of the detached portion of the tail was not that of the cometary particles forming the tail, it must be due to perturbations of these motions by meeting with a meteor swarm, or what is more likely, it is caused by a change in the rate of emission of the particles forming the tail.

The light-pressure theory seems to explain most easily the observed phenomena of the tails of comets. The above determined values of the magnitude of the repulsive forces give for the size of the small particles forming the three tails 0.1μ, μ, 1.33μ.[2]

COLUMBIA UNIVERSITY,
May 15, 1904

[1] ASTROPHYSICAL JOURNAL, **18**, 213, 1903.

[2] MITCHELL, U. S. *Monthly Weather Review*, July 1904.

Minor Contributions and Notes.

ESCAPE OF GASES FROM ATMOSPHERES.

A LETTER under the above heading, by Mr. S. R. Cook in *Nature* of March 24, puts forward views which ought not to remain on record without reply; and as between thirty and forty years ago I carried on the investigation into the rate at which gases can escape from atmospheres in the same way as Mr. Cook has done, and arrived from the premises employed by him at substantially the same conclusions, perhaps the best answer will be to state the considerations which led me to distrust that line of argument, and finally to abandon it. To do this, however, requires more to be said than can be brought within the compass of a letter to a weekly journal; and on this account and because the discussion is a physical discussion and concerns one of nature's greater operations, I venture to request the editors of the ASTROPHYSICAL JOURNAL to give space to the following pages, which are substantially as they appear in the current number of the *Philosophical Magazine.*

A study of the phenomena attending the escape of gases from atmospheres has been approached in two ways—*inductively*,[1] by arguing upward from events which are found to have occurred or to be in process of occurring in nature; and *deductively*,[2] by drawing inferences from the supposition that it is legitimate to attribute to the real gases of nature behavior which it has been ascertained would prevail in certain models of gas, so much simpler in their constitution than real gases that the progress of events within them is susceptible of mathematical treatment.

The two methods, as hitherto employed, have led to contradictory results, of which one at least must be erroneous. Mr. Cook, who has of recent years employed the deductive method, expresses the opinion in his letter that the numerical results which have been arrived at by this method

[1] G. JOHNSTONE STONEY, "Of Atmospheres upon Planets and Satellites." *Scientific Transactions of the Royal Dublin Society*, **6**, 305, October 1897; or ASTROPHYSICAL JOURNAL, **7**, 25, January 1898.

[2] S. R. COOK, "On the Escape of Gases from Planetary Atmospheres according to the Kinetic Theory." ASTROPHYSICAL JOURNAL, **11**, 36, January 1900.

G. H. BRYAN, "The Kinetic Theory of Planetary Atmospheres." *Philosophical Transactions*, A. **196**, 1, March 1900.

"will have to stand" until they can be disproved "by other *a priori* reasoning." Serious students of nature must, I think, hold that man, in his dealings with nature, is not in a position to limit in this way the kind of proof he will accept; and that it is sufficient if *in any way* Mr. Cook's inferences from Maxwell's researches can be disproved, whether by valid *a priori* or by valid *a posteriori* reasoning. And, moreover, that when once they are disproved we are brought face to face with the fact that there has been a mistake somewhere in the data which has led those who trusted in them to a false conclusion.

What convinced me several decades ago that the conclusion at which I arrived, and at which Mr. Cook has arrived, is false, is that it represents the Moon as incompetent to get rid of the atmosphere which it originally shared with the Earth, and of the gases which it has since evolved in abundance from its own interior. We knew thirty-five years ago, as we know now, that any reasoning which makes out that the Moon has retained its atmosphere must have a flaw in it somewhere. Furthermore, since that time, other facts not then known have come to light, and in a marked degree confirm the judgment which was then formed. Our confidence that we are on the right track is justifiably strengthened, when, as in this case further discoveries as they emerge confirm the view to which we had been led when our materials were more scanty. The presence of helium on the Earth was not then known; and the argument,[1] which has been based on what is now known of its behavior may be summarized as follows: Helium is supplied to the Earth's atmosphere through certain hot springs, and under circumstances which indicate that it also oozes up through the soil. It is, however, what is carried up by the water of these springs that can be subjected to experimental examination. The other gases of our atmosphere, such as nitrogen, oxygen, and argon, are found to accompany the helium in those springs; but with this marked difference, that whereas the other gases are present in such proportions as are consistent with their merely being portions of those gases which are being returned to the atmosphere after having been washed down into the Earth from the atmosphere by rain, the case is entirely different when we come to helium. The quantity of helium passed into the atmosphere through those springs is found to be from 3000 to 6000 times more than can be accounted for as a return to the atmosphere of helium which had been washed down out of it. Accord-

[1] The argument here summarized is based on the marvelous determinations made by Sir William Ramsay, or in his laboratory, and will be found with the necessary details in a paper "On the Behavior of Helium in the Earth's Atmosphere," by G. JOHNSTONE STONEY, ASTROPHYSICAL JOURNAL, 11, 369, 1900.

ingly we are justified in regarding this great surplus of helium as being an addition which is being uninterruptedly made to the atmosphere. Notwithstanding this, the quantity of helium in the atmosphere has not gone on increasing. The Earth at the present rate of supply furnishes in a small number of years a quantity of helium equal to the quantity which the atmosphere can at present retain—*i. e.*, in a number of years which is exceedingly small from a geological standpoint, which is the point of view that is here appropriate. The inference from these facts is the obvious one, that helium is by some agency being eliminated from our atmosphere as fast as it is being introduced into the atmosphere from the Earth. Two possible agencies for the elimination of the helium suggest themselves: chemical reactions and an escape of helium from the upper part of the atmosphere. Of these, chemical agency is excluded by the extreme chemical inertness of helium. What remains then is that there is an outflow of helium from the top of the atmosphere equal to the inflow at the bottom, and that the trace of helium which is at any one time present in the atmosphere is helium part of which is slowly making its way upward to the situation from which some of its molecules can escape, and so produce that outflow which balances the net influx at the bottom of the atmosphere.

Having satisfied myself that the deductive method as I applied it (and as Mr. Cook has applied it) lands us in erroneous results, I set to work to scrutinize the data of the deductive argument with a view to ascertaining how far they may be depended upon, and at what points they are doubtful. All branches of physics require us to be more or less on our guard against trusting without sufficient scrutiny to inferences from that mixture of theory and hypothesis of which we are obliged to make use in order to be able to employ mathematics in physical research. The demand for this caution becomes a pressing one when, as in gases, we are obliged to deal with immens numbers of events, *each of which has its own dynamical history with incidents peculiar to itself*, and where what chances on some of these occasions differs enormously from that which occurs in most of them. Of this kind are the interactions between the molecules of a gas and the interfused ether, and especially those complicated struggles between molecules which we call their encounters—events each of which, when viewed, as it ought to be viewed, from the molecular standpoint, is a battle lasting a long time, as time has to be measured in molecular physics, and with an immense number and variety of incidents. These—the interactions between the molecules and the ether, and the interactions between molecule and molecule—are the primary events, the real determining events, which occur within a gas; while the movements of the molecules as

they dart about between one encounter and the next; the spectrum radiated by the gas; the ions which present themselves after some of the encounters; the compounds which result from chemical reactions during some of the encounters (if what we are dealing with happens to be a mixture of suitable gases); and finally that remarkable partition of energy between the events going on within the molecules and the translational motions of the molecules, which is effected during some of the encounters—all of these are subordinate events depending upon those which are above spoken of as the primary events. When dealing with such almost immeasurably intricate and obscure operations of nature, it behooves us with the very utmost caution to distinguish between what is theory and what hypothesis in the data we employ, in order to be able to ascertain how far any conclusions we draw follow from the one, and how far they involve the other with the risks inseparable from it.

Theories are suppositions we hope to be true; hypotheses are suppositions we expect to be useful. As to theories, they are either correct or erroneous. They may be, they usually are, but they by no means need be, of use to man. The virtue of a theory is simply to be true. On the other hand, hypotheses usually makes use of machinery which we can see to be simpler than that operating in nature; and especially is this the case with those hypotheses to which we are obliged to have recourse in mathematical investigations, which, in order to be of use, must be so great a simplification of the complex intricacies of nature that human mathematics shall be able to cope with them.

The *theory* of gas universally put forward in scientific books when the present writer was young was the erroneous statical theory that the molecules of a gas may be stationary, that they have a capacity for expanding and contracting, and that each molecule presses against its neighbors. An illustration frequently made use of in those days was that of a froth of bubbles pressing against one another. This erroneous theory held the field in Avogadro's time and for more than thirty years afterward; but in the fifties of the nineteenth century it was gradually, though not without protest, displaced (chiefly through a masterly series of papers by Clausius) by the kinetic theory, which is now the prevalent theory. The kinetic theory of gas, as formulated by Clausius, regards the molecules of a gas as missiles of equal mass, darting about in space and not acting *sensibly* on one another except when "encounters" chance to take place; *i. e.*, not until the centers of mass of two molecules get within an interval of one another which is less —usually much less—than the average length of the free paths which the molecules describe between the encounters; which free paths are accordingly

approximately straight and pursued with unvarying speed, except so far as they may be slightly influenced by gravity or other external cause, or by some excessively minute part of the interactions between molecules, if any such survives when the interval between molecules gets beyond what we may call their encountering distance.

This is the kinetic theory of gas as put forward by its founder,[1] and any system of bodies which conforms to this definition may be called *a kinetic system.* Thus, there are in nature as many kinetic systems as there are distinct gases; and, moreover, all those models of gas in which the progress of events has been studied by mathematicians are *each of them* a kinetic system. So also are the cosmic bodies of celestial space, if we eliminate from the definition the condition that the masses must be equal; and, in fact, some modification of this clause of the definition is essential even as regards gases, inasmuch as in all the gases of nature there are found some of the missiles differing in mass from others—thus, in diatomic gases ions present themselves with masses that seem to be half the mass of average molecules.

We may add further details without trespassing beyond the domain of theory, *i. e.*, while still endeavoring to describe events as they occur in nature. Thus we may add that elaborate internal events are going on within all these missiles, which internal events absorb about one-third of the whole available energy of the gas; and we know that two partitions of energy take place—one a partition of energy (which probably goes on uninterruptedly) between these internal events of the molecules and the events of the ether, the other a partition of energy which now and then occurs with comparative suddenness between the internal events of the molecules and their translational motions. This latter transfer of energy seems to take place only when two molecules are in grip with one another during an encounter, and not at every encounter, but only during those which take place under certain necessary conditions. If, as seems probable, encounters with these special characteristics are as rare as those which result in the breaking down of molecules into ions, or of those which result in chemical reaction in a mixture of equal volumes of chlorine and hydrogen, then the infrequency of their recurrence can be estimated; and, in cases in which it has been found possible to make the estimate, the infrequency seems to range

[1] Clausius' papers were preceded by a paper by Waterston which was presented to the Royal Society in 1845, but which was not then published. This paper, when it long afterward came to be printed, was found to contain a most valuable anticipation of the kinetic theory as developed by Clausius. If Waterston's paper had been printed in due course, the kinetic theory would probably have been adequately dealt with some years sooner.

from one out of 10^9 encounters down to about one in 10^{15}, when we pursue the observations so far as they have been recorded.

It is here that I strongly suspect, though I am not in a position to claim that I know, that the mistake has been made by Mr. Cook, and by my friend Professor Bryan, who both tacitly assume that this partition of energy is a process which goes on uninterruptedly, even in the upper parts of the atmosphere. Whether the mistake be here or elsewhere may as yet be only highly probable; but that a mistake exists *somewhere* in the premises of the deductive argument was placed beyond question by nature when she presented to us events that have occurred, or are occurring, which negative some of the inferences to which those data lead. We may be unable with certitude to put our finger upon the precise spot where the mistake came in, but that a mistake has come in somewhere can be proved.

When Maxwell determines his law for the distribution of speeds in a kinetic system, he exercises a caution[1] which has not always been observed by his successors, and is careful to present the law as the law governing the distribution of speeds (not in every, or indeed in any, gas, but) in a kinetic system which consists of numberless equal particles, each of which is a perfectly rigid and perfectly elastic sphere, after an immense number of collisions have taken place—assumptions which he afterward varied in different ways, as by substituting particles of other forms, or points repelling one another inversely as the fifth power of the distance. The several assumptions which he thus makes are put forward, not as theory, but as hypothesis; they do not profess to reproduce any existing gas, but substitute for the gas an artificial model; and Maxwell is careful to keep this prominently before the mind of his reader.

As to his exponential law for the distribution of speeds, it is the solution of a functional equation, which in turn is the expression of the assumption that the number of molecules whose velocities lie between u, v, w, and $u+\delta u$, $v+\delta v$, $w+\delta w$ *must be some function of* u, v, *and* w. Now, this is true of Maxwell's models, but cannot be the case in any gas in which there is an irruption of energy from the internal motions to the translational on the occurrence of events which depend either wholly or partly on conditions other than the mere translatory speeds of the molecules—such conditions, for example, as the aspects of the two molecules to one another when the encounter is about to take place, or the phases at which the internal motions had arrived at that instant of time, or many other conditions that are possible and can easily be conceived. Accordingly, whenever a mathematician applies Maxwell's law under the impression that, as regards any

[1] See MAXWELL'S *Scientific Papers*, I, 380; or *Phil. Mag.* for January 1860.

particular gas, it is more than an approximate law, he tacitly assumes either that there are no internal events (as in Maxwell's models), or that, if there be internal events (as in all real gases), the partition of energy between these internal events and the translational motions is a transfer taking place at such short intervals that it may legitimately be treated by the mathematician as a process which goes on continuously and at a constant rate. At the bottom of our atmosphere an event that happens once in 10^9 encounters occurs to each molecule as often as seven or eight times per second. Even here the assumption that the transfer of energy goes on uninterruptedly makes but a rough approximation to the truth, and it is utterly remote from being an approximation in that penultimate stratum[1] of the atmosphere from which nearly the whole escape of molecules takes place, and especially in regard to an event like the escape of a molecule from the Earth, which is mainly the outcome of the circumstance that an individual encounter has chanced to be very unlike ordinary encounters. Hence, in no real gas can the actual law of the distribution of speeds be *identical* with Maxwell's exponential law, nor with any of the exponential laws of Maxwell's successors; although under the conditions which prevail in our laboratories these laws may be an approximation sufficient for many useful purposes.

The cases in which Maxwell's approximate law may legitimately be employed can be pointed out. Whenever an approximate law presents itse f in an exponential form with negative index, the approximation holds good as an approximation over that small part of the range where the exponential function acquires large values, but can no longer be depended upon as an approximation in regard to the parts of the range where the exponen-

[1] If we conceive the helium in the Earth's atmosphere to be divided into horizontal strata A, B, etc. . . . . X, Y, Z, beginning at the bottom of the atmosphere, then Z the outermost of these, may be defined as the layer which is characterized by the presence of helium molecules which, having been thrown up from below, usually—say, in nine cases out of ten—fall back without having met with an encounter in stratum Z, Y, the penultimate stratum, may be defined as that layer within which most of the molecules thrown up into the ultimate stratum, Z—say, nine out of ten of them—had met with their last encounter, and within which most of those which fall back from stratum Z will meet with their next subsequent encounter. Molecules flying upward after an encounter in the next underlying stratum, the ante-penultimate stratum X, will occasionally, although but seldom, escape encounter in stratum Y, and so get direct from X into Z.

Accordingly, the molecules which succeed in escaping from the Earth must have come in most cases direct from a favorable encounter within the penultimate stratum Y, in a few cases from an encounter in either X or Z, and but very seldom indeed from any of the strata farther down than the ante-penultimate stratum X. These outer strata, especially Y and Z, must be of immense depth.

tial function is small. Maxwell makes a legitimate use of his law when, through its instrumentality, he discovered his remarkable explanation of viscosity and diffusion, and investigated the laws of those phenomena. In reference to these, what happens in the case of velocities which are infrequent is of small account; but the application made by Professor Bryan and Mr. Cook is to the rare events which occur within that part of the range where the approximation breaks down, and where, in consequence, the exponential law is misleading. It is this oversight to which I think it likely that we are mainly to refer numerical results which are found to clash with events that have taken place or that are taking place upon the Moon and the Earth.

The inquiry in which I engaged in the sixties of the last century led also to the detection of other defects in the premises made use of by those who have trusted in the deductive method. One of these concerns the ambiguities which surround the use of the term "temperature." Temperature is not one physical measurement, but two groups of physical measurements, essentially different according as we test equality of temperature by there being no transfer of heat *by conduction* when two bodies are brought into contact, or *by radiation* when they are made to stand apart. This establishes a division of temperatures into two principal groups, and these groups require further subdivision. The temperature of a body determined in these two different ways may be called its conduction temperature and its radiation temperature; of each of which there are several varieties. There are accordingly many different kinds of temperature. In the case of gases, conduction (including convection) is mainly concerned with the translational speeds of the molecules, while radiation in the first instance affects only the internal events going on within the molecules. In most laboratory experiments carried on as they must be at the bottom of our atmosphere) the partition of energy between the internal events of each molecule and its translational movements takes place so frequently—probably several times every second in a gas at standard temperature and pressure—that the distinction even between the two main kinds of temperature does not need to be attended to. But, to go to the opposite extreme, let us consider the case of a gaseous molecule which has escaped from the Earth and travels like an independent planet through space. Here no interchange of energy can take place between the translational movement of the molecule and its internal events. Under suitable external influences either of them may be made to vary to any extent without this affecting the other. The two kinds of energy, or if we please so to call them of temperature, have become divorced; and intermediate stages between these extremes would be found

to exist within an atmosphere if we could explore it from its bottom to its top.

Further distinctions have to be made within the two principal kinds of temperature. Those which have to be taken into account in the present investigation are the varieties of radiation temperature. A body, like the Sun, acting by radiation upon different gases has no one definite radiation temperature, but may be at a different radiation temperature in regard to each gas. Thus, the Sun is hotter with regard to the helium of the Earth's atmosphere than with regard to its hydrogen. This we know, because the radiations from the Sun which can affect hydrogen come in the form of the rays corresponding to the hydrogen lines of the solar spectrum which are dark, while the radiations which raise the temperature of helium come through rays corresponding to the helium lines, of which the principal one within the visible spectrum—the double line D_3—is as bright as the neighboring part of the spectrum. Hence the radiation which reaches helium in the outer part of our atmosphere has the full intensity of radiation from the Sun's photosphere.

Reviewing the whole case, we find that in the stratum of the Earth's atmosphere from which helium escapes, the opportunities for exchanging energy between the internal motions and the translational, instead of occuring to each molecule several times per second, may be so infrequent that they occur only once in several hours. During all its intermediate flights the molecule is exposed during the daytime to the full glare of radiation as intense as direct radiation from the Sun's photosphere. In this way the internal motions of the molecule will be kept for some hours excited to intense acti ity, and if during these hours that special kind of encounter happens to take place which affords an opportunity for an interchange between the internal and translational energies, the two encountering molecules will fling asunder with what may be described as explosive violence. All that is then necessary for a molecule to escape is that one of the two that have encountered shall have the direction of its flight outward, that it shall have sufficient speed, and that it shall escape other encounters. If the chance that these events shall concur befalls each molecule in the penultimate stratum of the helium atmosphere as often as once in several days, there would probably be an abundant outflow of helium from the Earth to account for the observed rate of its escape.

Here, however, we are on debatable ground. We can only follow the individual events of molecular physics with probability, not with certainty. But, on the other hand, when we trust to the inductive argument based on the ascertained behavior of helium, as stated in an earlier paragraph, *we are on secure ground.* We may rely on the conclusion to which it leads,

viz., that helium *is* escaping from the Earth's atmosphere, and that the rate of escape is the same as the rate of the net inflow from the Earth into the atmosphere. By the net inflow is meant the supply after deducting something like a 1–6000 or 1–3000 part of the whole, in order to allow for the very minute quantity of helium that had been washed out of the atmosphere by rain and which is being restored to it.

There are other matters, too, which would need to be understood and allowed for before we should be entitled to trust the deductive method of proof. Thus, the internal events that go on within the molecules of matter are of more than one kind, and in gases stand differently related to the translational motions. This is revealed to us by phosphorescence and other phenomena. An attempt to make a preliminary classification of these internal events has been made by the present writer in a memoir on the kinetic theory of gas.[1] But without going into these and other matters enough has been said to show how inadequate the deductive method is—at least as hitherto handled—to be a safe guide in dealing with the matters with which it has been made to grapple. This of course also shows that objections based on investigations of this character have no weight against the testimony about the rate at which gases do actually escape from atmospheres which is given by such *facts* as the absence of atmosphere from the Moon and the behavior of helium upon the Earth.

The objection urged by Mr. Cook against accepting the inductive proof of the actual rate of escape of gases from atmospheres is analogous to the objection urged by some scientific men when in 1867 I brought forward a proof[2] that in an atmosphere of mixed gases the atmosphere of each gas must have a different limit, the lighter constituents overlapping and extending beyond those which are denser. "Oh!" it was then said, "that can't be the case. It is inconsistent with Dalton's law of the equal diffusion of gases." Yet I have lived to see my conclusion generally, I believe universally, accepted by physical astronomers; and I look forward with some hope to an ultimate acquiescence in what is now being objected to, in reference to the escape of gases from atmospheres. In both cases the objection rests on the same error—the mistake of hypothesis for theory, and the consequent mistake of a law which is approximate for a law of nature.

G. JOHNSTONE STONEY.

30 LEDBURY ROAD, LONDON, W.,
May 12, 1904.

[1] "Of the Kinetic Theory of Gas regarded as Illustrating Nature," *Scientific Proceedings of the Royal Dublin Society* of June 1895, **8**, 356, or *Phil. Mag.*, October 1895, p. 362.

[2] "On the Physical Constitution of the Sun and Stars," *Proc. R. S.*, No. 105, p. 1 (1868). See especially paragraphs 23, 24, 25.

NOTE ON THE PARALLAX OF *NOVA PERSEI*.

In the March number of this JOURNAL Mr. Otto Luyties asserts[1] that my computation of the parallax of *Nova Persei*[2] is incorrect, because "it involves two tacit assumptions, of which one is erroneous." But the assumptions named were not made by me, even tacitly, since my points of reference were not taken at "the outer limits of the apparent illumination." The positions actually chosen were at centers of greatest intensity, where the objections made do not apply.

Consequently my conclusions as to the parallax of *Nova Persei* and as to the relative masses of the ions are in no wise invalidated by a correct application of the principle elucidated in Mr. Luyties' article.

FRANK W. VERY.

WESTWOOD, MASS.,
June 3, 1904.

[1] ASTROPHYSICAL JOURNAL, **19**, 130, 1904.

[2] *American Journal of Science*, **16**, 127, 1903.

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